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USTRALASIAN JOURNAL OF PHILOSOPHY

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I. ROBERT HOFFMAN. II. D. H. MONRO.
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CRITICAL NOTICE:

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REVIEWS, BOOKS RECEIVED, NOTES AND NEWS.

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THE AUSTRALASIAN JOURNAL
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Vol. 37 May, 1959 No. 1

MEASUREMENT[1]

By J. J. C. Smart

The word 'measure', it will become apparent, is used in many different, though related, senses. For example, measurement of length is a very fundamental sort of measurement: using a stick as standard, and by explaining what 'equal in length' means by putting sticks side by side and what 'addition of length' means by putting sticks end to end, a man on a desert island could build up and teach a system of measurement of length to a set of scientifically illiterate people. On the other hand, measurement of the charge of an electron is something which could only be done by, and taught to, people in possession of a considerable body of experimental and theoretical physics. Different in another sort of way again is measurement of temperature. There is a sense, I suppose, in which we can say that a temperature of 200° absolute is twice a temperature of 100° absolute, but it is an odd sort of sense. We cannot put temperatures end to end in the sort of way we can put bits of wood end to end. Different again is the sense of 'measurement' used by psychologists, where the only means used to assess test scores is counting, with the results of counting manipulated arithmetically through considerations arising from the theory of probability. We can think of the various uses of the words 'measurement' and 'magnitude' as related by a sort of 'family resemblance' (to use a simile of Wittgenstein's). We can think of them as like parents, sons, and daughters, grandsons and granddaughters. We may also come across cousins, second-cousins, great-uncles and great-aunts, about whom it will be somewhat arbitrary whether we include them in the family or not. (Psychological measurement, for example, is such a distant relation, and hence the inconclusiveness and futility of many discussions of whether psychological measurement is 'really measurement' or not.)

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[1] Based on a paper read at Section J of ANZAAS, Melbourne, 1955

Some uses of the word 'measurement', as I have suggested, presuppose others. Let us consider the sort of measurement that presupposes no other sort, the sort of measurement that could be taught to a savage on a desert island. Measurement of length, mass, and periods of time are some (though not all) of the kinds of measurement of this bedrock sort, which N. R. Campbell (whose ideas on measurement I shall make considerable use of) appropriately called 'Fundamental Measurement'.[2] (Though I deviate from Campbell's terminology slightly. For example, what I shall call a 'fundamental defined magnitude' is treated by Campbell as a 'derived magnitude'. This is, I think, because he is insensitive to the distinction between logically necessary and empirical propositions..)

Before teaching a savage the concept of length or mass it would be necessary to teach him to count, and so if we regard counting as measurement then counting is the most fundamental sort of measurement of all. (Though 'count', like 'measure', is a polymorphous concept. Compare 'count the apples' with 'count the roots of the equation'.) Let us, however, assume that the savage has already been taught to count and to do elementary arithmetic.

When we say that a property is a fundamental magnitude we mean that there is a procedure of fundamental measurement which determines it. What criteria must a property satisfy to be of this sort? First of all, the property must be such that it enables us to put things possessing it into a unique *order*. Thus, to use a common example, the relation of *scratching* would satisfy this criterion if it were the case that (a) if A scratches B and B scratches C then A scratches C and (b) if A scratches B then B does not scratch A. These two conditions would imply that if A scratches B and B scratches C then C does not scratch A. In actual fact this is very nearly true, and we can thus arrange substances in an order of hardness, saying that they are of the same hardness if neither scratches the other, and that if A scratches B then A is harder than B However there are exceptional substances such that one of them A scratches a second one B, and B scratches a third one C, and yet C scratches A. In so far as these cases exist it is not strictly true that the relation of scratching determines a scale, that is, an order of hardness. Notice, therefore, that for an operation such as scratching to determine an order it is necessary for certain empirical facts about the world to be true. We cannot tell *a priori* whether an operation determines an order, though of course sometimes we may need only very familiar facts about the world to assure ourselves that it does.

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[2] *Physics, The Elements*, Part II, Cambridge University Press, 1920 *An Account of the Principles of Measurement and Calculation*, Longmans, 1928. See also the improved treatment by Hempel in 'Fundamentals of Concept Formation in Empirical Science', pp. 62-78, *International Encyclopedia of Unified Science*, Vol. II, No. 7, University of Chicago Press, 1952.

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Neglecting the complication arising from these few anomalous substances that do not obey conditions (a) and (b) of the last paragraph, the relation of scratching enables us to place substances in an order of hardness. We could represent the hardness of substances by assigning numbers. Notice, however, that though we can assign numbers to represent degrees of hardness, it is arbitrary which numbers we use, so long as they are in the right order, that is, with the greater number to the greater hardness. For example, we could assign 1 to wood, 5 to glass, and 10 to steel, but we could just as well assign 3 to wood, 5 to glass, and 7 to steel. It thus makes no sense to say that steel is or is not twice as hard as glass, or even that the difference in hardness between steel and glass is or is not the same as the difference in hardness between glass and wood. We have got an order, or scale, of hardness, but we have not got a magnitude. That degrees of a property can be put in an order is a necessary but not a sufficient condition of the property's being a magnitude.

Masses, lengths, and periods of time can clearly be put in an order. For it is an empirical fact that if a mass A is put in one pan of a balance and a mass B is put in the other pan and A goes down, and if B is weighed against C and B goes down, then A will go down when weighed against C. Also that if A balances B then, if A goes up (or down) when weighed against C, B goes up (or down) when weighed against C. (What is a balance? Roughly, it is a thing for which certain empirical conditions are true, such that if A is put in the left hand pan and B is put in the right hand pan and they balance, then they will also balance if A is put in the right hand pan and B in the left hand pan. For the moment let us disregard the difficulty that these criteria for being a balance are always only approximately satisfied.) Similarly, things can be put in an order of length by means of the relation of overlapping. It is an empirical fact that most things are not like india-rubber: things like pieces of elastic apart, if A is put against B and overlaps it, and if B is put against C and overlaps it, then if A is put against C it will overlap it. Similarly, once more, with periods of time. 'The time taken to boil an egg (process A) is less than the time taken to roast a chicken (process B), which again is less than the time taken to eat a horse (process C)', can be taken to mean that if processes A, B, and C start at the same moment then A finishes before B and B before C. It is an empirical fact that processes can be found of which such conditions are invariably true. (Our example was a bad one in so far as there can be slow chicken-roasters and fast horse-eaters.) Provided that some such processes can be found, we can define an order in which periods of time can be put.

In order to get a *magnitude,* not just an *order,* we need some physical process corresponding to addition. Putting two things in the same pan of a balance is such an operation. For, denoting this operation by $+$, we have such facts as that if $A + B$ balances C, and $C + D$ balances E, then $(A + B) + D$ balances E. It would be tedious to go through the commutative, associative and distributive laws of addition in detail, but they all hold (approximately at any rate). Similarly, putting end to end is a physical operation that defines addition of lengths, and making one process start immediately another has stopped defines addition of periods of time. With hardness there is no operation with the required properties, and addition of hardnesses has no meaning.

Taking a mass S as a standard, any object that balances S is said to have a mass 1. Any object that balances two objects that each balance S is said to have a mass 2. An object that balances an object that balances two objects that balance S together with an object that balances S is said to have a mass 3. Two objects that balance one another and which together balance S are said to have each a mass $\frac{1}{2}$. And so on. The (rational) numbers which we assign to any given mass are not now arbitrary, or, more precisely, they are arbitrary only to the extent that it is arbitrary what object we choose for our standard—our unit mass. Any two systems of measurement of mass will differ only in that if M is the mass assigned in one system, kM will be assigned in the other system, where k is some constant.

Notice that fundamental measurement really is fundamental. In order to teach a savage what 'twice as long' means, you do not need first of all to teach him what it is to be twice as something or other else. (*A fortiori* you do not need to teach him any numerical laws of nature.) All you have to do is to play around with bits of wood, teach him what 'longer than' means, and teach him that an object A has the same length as an object B if they fit end to end, and that an object C has the length of A plus the length of B if B on the end of A gives us an object with the same length as C. Then, if he knows about fractions, you can select some piece of wood as a standard and teach him what a length of $5\frac{3}{4}$ is.

When we have our fundamental magnitudes we can define others in terms of them. For example we can define average velocity as the quotient of distance travelled by time taken to travel. That is, average velocity is defined in terms of two fundamental magnitudes. It is clear that, though fundamental measurement must always give us quantities expressible by positive rational numbers, negative, irrational, and even transcendental numbers may be required to express defined magnitudes. For example we could define y in terms of x as $\sqrt{x}$ or $\log x$. Sometimes the use of mathematics, which is analogous in some ways to a system of interlocking definitions, may make it necessary for us to use irrational and transcendental numbers even when ascertaining our original fundamental magnitudes. Suppose that a primitive people develops a system of fundamental measurement of length, and that, more intellectual even than the Greeks, these people develop systems of Euclidean and non-Euclidean geometry. They then make measurements of the lengths of the sides and diagonals of various squares and decide that Euclidean geometry is the one which is best applicable to reality. They then say that the length of the circumference of a circle of unit radius is 2π. So transcendental numbers are needed, though our primitive people have not defined any magnitudes not determinable by fundamental measurement. It is possible also by definition to define what are in reality quite new sorts of magnitudes. Suppose a primitive people has developed the fundamental measurement of mass and also a crude statics, in which they define the magnitude of a force in terms of the weight needed to balance it, and in which they define weight as proportional to mass. Unknown to them, however, this definition depends for its usefulness on a happy accident, the constancy, in the regions in which our tribe conducts its experiments, of the earth's gravitational field. The definition is not so stipulative as it looks. It is not *purely* stipulative, as is the definition of average velocity. This leads us to a type of magnitude whose possibility depends, quite openly, on the truth of certain numerical laws connecting other more fundamental magnitudes.

Such magnitudes are *derived magnitudes*. An example given by Campbell is *density*. The mass of a substance like water varies directly with the volume it fills. That is, $m \propto V$ or $m = \rho V$.

The number ρ varies according to the substance in question. The law $m=\rho V$ enables us therefore to define a new quantity, density, in such a way that a non-arbitrary number is assigned to a substance (non-arbitrary, that is, once the units of m and V are decided on). m and V are fundamental magnitudes, or alternatively V might not be itself a fundamental magnitude, but a fundamental defined magnitude (defined in terms of the fundamental magnitude *length*). This example of Campbell's may by some be found unconvincing. For it is difficult to know whether '$m=\rho V$' is a numerical law: it is suspiciously like an analytic proposition. For the procedure of adding masses (putting them together in a balance) whereby mass is defined is also a procedure of adding volumes. Nevertheless it might be that we could put masses of a certain substance together in such a way that they coalesced, and that equally we could separate them at will. If so we could define 'mass' in the usual way and '$m=\rho V$' would be false for this substance. Of course, this substance could not be used for our standards of length. m and V, it must be noticed, are fundamental magnitudes: they are defined purely phenomenologically. Later on we shall find m functioning as a theoretical magnitude. and in so far as it does so laws in which it enters may approach nearer to being analytic though they will never become quite analytic.

Once we have a number of derived magnitudes we can, of course, derive other magnitudes from them at a further remove. Let p be the pressure of a gas, v its volume. The relation $pv =$ constant enables us to use this constant as the measure of a new magnitude, absolute temperature. Again, suppose that we have already in some way defined current and potential difference. Then $\frac{\mathrm{E}}{\mathrm{I}} =$ constant (Ohm's law) enables us to define a new magnitude, electrical resistance.

We see that in order to introduce a derived magnitude we must already have established one or more numerical laws relating previously introduced magnitudes. These laws are experimental laws: so far we have not considered theoretical laws, and theoretical magnitudes.

For a derived magnitude there need be no physical operation corresponding to addition. It makes no sense to talk of adding temperatures or adding densities, and yet temperature and density can be introduced as perfectly good derived magnitudes.

There is a sort of magnitude that is somewhat intermediate between being a defined fundamental magnitude and a derived magnitude. Suppose that we are with our savage again, and that we have taught him the concept of length. We could then produce a uniform thin glass tube with a bulb on the end, fill the bulb with mercury, and mark the level of the mercury when the bulb is in melting ice '0' and the level of the mercury when the bulb is in boiling water '100', and then divide the intervening part of the tube into 100 equal divisions. This would give a meaning of a sort to 'a temperature of 70°' (indeed historically this is the form in which the notion of temperature was first introduced), but this procedure gives us a rather distant relation in the measurement family. How do we know, so far, that, if we made a thermometer using some liquid other than mercury, our two thermometers would give approximately the same readings in the same circumstances? The whole procedure is arbitrary. It does not depend on a previously established numerical law, as does the gas scale of temperature. Why should we not have made our gradations logarithmically, or perhaps, indeed, according to some quite odd mathematical law? (Perhaps a different law according to whether mercury or some other liquid is used.) That the procedure of defining temperature in this sort of way was fruitful is, from the point of view of logic, a happy accident.

The gas scale of temperature, so long as we only need it to be approximate, is a derived magnitude. (Theoretical considerations involving the concept of an ideal gas may, of course, be needed to refine it.) There is another scale of temperature, the thermodynamic one, which is of yet another sort. Temperature, so defined, is a theoretical magnitude. Another example of a theoretical magnitude is *force*. (In fact force is not a scalar magnitude but a vector, that is an ordered set of magnitudes, but this distinction need not concern us here.) Admittedly there might be a crude concept of force as a fundamental defined magnitude in an elementary and rough and ready investigation of statics, but force as it occurs in dynamics cannot be so defined. Dynamics as we know it today was first introduced to deal with the motions of the sun, moon, and planets. We determine the positions, velocities, and accelerations of the planets and in accordance with the theory of dynamics we deduce propositions of the form 'the force on such and such a planet is F'. The meaning of the word 'force' can be explained by explaining the functioning of the word in dynamics, but outside dynamics it is without a function. That is, statements about dynamical forces are meaningful only in the context of a theory. Similarly, gravitational, electric and magnetic field, wavelength of light, and nuclear charge are examples of theoretical magnitudes. If we open a book on physics we shall find that most of the magnitudes mentioned are theoretical magnitudes. Even more than derived magnitudes, they are sophisticated concepts. Before we could teach a savage what a certain derived magnitude was, we should have to get him to understand what certain fundamental magnitudes were, and we should have to demonstrate to him certain numerical laws relating these fundamental magnitudes. In order to teach a savage what a theoretical magnitude was, we should not only have to get him to understand, first of all, fundamental measurement and probably also a few numerical experimental laws and some examples of derived measurement, but we should also have to teach him a theoretical hypothesis, the statements of which, in conjunction with observation statements, would enable him to deduce further observation statements. The theoretical statements, however, cannot be asserted in isolation from one another, but only as part of a system, and statements assigning the values of theoretical magnitudes in special cases (say the gravitational force on the moon) would themselves be meaningless unless regarded as moves in the deductive game and not isolable from it. (Though I do not want to say that a theory is just a deductive game.)

We have made a few distinctions, and it is necessary to make them if we wish to be clear about scientific magnitudes. However, by a sort of feed-back process, which is very characteristic of science and which occurs over and over again in different ways, these distinctions get blurred as science proceeds. For example, mass is not entirely a fundamental magnitude, though it started, and had to start, as such. For accurate work we make corrections of our results of weighing by using theoretical considerations from mechanics and hydrostatics. And 'mass' in celestial mechanics is of course a theoretical concept. The picture can get blurred in other ways. Electric current was originally defined as a theoretical magnitude from which we can deduce the force on a nearby magnet (and force itself is a theoretical magnitude). Or, even more reconditely, electric current can be defined as rate of change of electric charge, and electric charge was originally an electrostatic concept. Maxwell's theory then enables us to deduce from propositions about the rate of change of electric charge certain observable consequences about the motions of magnets and of other wires containing currents. Having a definition of electric current, and setting up in some way a suitable definition also of potential difference, Ohm's Law enables us to introduce electrical resistance as a derived magnitude. (In this case we might call it a derived theoretical magnitude, as at least one of the magnitudes on which its definition *via* Ohm's Law is based is a theoretical magnitude.) On the other hand, electrical resistance could be introduced differently, namely as a fundamental magnitude. (There is a suitable physical operation corresponding to addition: putting in series.) Now suppose someone asks whether resistance is a fundamental magnitude or a derived one? There is no answer. From the point of view of theory it is more simple to define electrical resistance in terms of current and potential difference. From the practical point of view, however, resistance is much easier to measure directly than current or potential difference. So it is convenient to treat resistance sometimes as a fundamental magnitude and sometimes as a derived magnitude. A further feature which complicates the logical situation may be mentioned. If we measure resistance directly as a fundamental magnitude we do it by means of a Wheatstone bridge circuit. We arrange resistances in such a way that we get a null current through a galvanometer. Furthermore, we adjudge certain potential differences to be equal. Since to ascertain that a current is null need not be to make use of a system of *measuring* current, and since to adjudge certain potential differences to be equal can in this case be done on theoretical grounds without actually measuring these potential differences, resistance as measured by the Wheatstone bridge is not a derived magnitude (does not depend on the previous ascertaining of other magnitudes). But, though the Wheatstone bridge method could be taught to a savage as a system of fundamental measurement, as so taught it would seem to have an element of arbitrariness. This arbitrariness could only be removed by bringing in theoretical considerations. Hence, though there are good reasons for regarding resistance, determined by this method, as a fundamental magnitude, there are also reasons which would incline us to classify it a theoretical magnitude. An adequate taxonomy of measurement would be almost impossibly complex. Here we shall merely endeavour to indicate the sources of complexity and to understand them.

In order to see further how the distinction between theoretical and non-theoretical magnitudes gets blurred, let us revert to the concept of length, which is a good example of a magnitude which is sometimes a fundamental one, sometimes a theoretical one, and sometimes a bit of both. First of all length has to be defined as a fundamental magnitude (remember our savage on the desert island). We also define other fundamental magnitudes and discover numerical laws relating these, and furthermore we can define derived magnitudes and investigate further experimental laws relating these new magnitudes. We then invent abstract theories to explain these laws, and the magnitudes involved in the theory will be theoretical magnitudes. We can then use some theoretical standard as our standard of length, for example the wavelength of cadmium light. Nevertheless we must not forget that (a) it would be impossible to *begin* with this standard of length: measurements based on, say, the standard metre in Paris were needed before we could get the experimental evidence for our theory; and (b) we must have some concept of length which enables us to give meaning to the statement that the diffraction grating of the spectrometer has so many lines (evenly spaced) on it. That is, the wavelength of cadmium light may be used as a test for the correctness of our scales (even the scales on our spectrometer themselves), but without the scales in the first place we could never test the length of anything by our theoretical standard. The method is clearly one of successive approximation.[8] Our measurements of length may be modified further by means of the theory of errors. This is not a theory in the sense in which we ordinarily speak of a scientific theory: it is really a theory of pure mathematics (that is, it is a theory in the sense in which we speak of 'theory of functions', not in the sense in which we speak of 'quantum theory'). The mathematical theory of errors can be used to correct our measurements as follows. Sometimes the measurements we make, let us say by fundamental measurement, are 'subject to error': what this means is that beyond a certain degree of accuracy they do not give rise to simple experimental laws. The hypothesis of random or quasi-random error is introduced, and the readings are reduced to ideal measurements. What we measure in this way may be, near enough for our purposes, regarded as fundamental measurement. But it is not quite fundamental measurement. It could not be taught to our savage on the desert island without teaching him first the theory of errors (though this hardly counts as it is a branch of pure mathematics) and about empirical laws: he would have to learn the manner in which our original uncorrected measurements nearly, but not quite, fit these laws and how, if we correct these measurements in a certain way, they will fit them. So our resulting magnitudes are not prior to all experimental laws whatever, as true fundamental magnitudes are.

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[8] See Lenzen's: 'Procedures of Empirical Sciences', *International Encyclopedia of Unified Science*, Vol. I, No 5.

The mention of the theory of errors naturally leads us to the question of what sort of things probabilities are. We can give probabilities precise numerical values, and when we say 'the probability of this badly made die falling with the 6 uppermost is 0·1357', the number 0·1357 is certainly a magnitude. It is, however, a theoretical magnitude. There are no empirical operations which give us the number 0·1357, in the way that weighing gives us the value of a mass: there is only a series of empirical tests whereby we can verify the *hypothesis* that the probability is 0·1357. Thus four sets of 10,000 throws, which gave the values 1357, 1358, 1357, 1356, would be regarded as verifying the hypothesis. If we got 1350, 1307, 1355, 1309 we should reject the hypothesis that the probability was 0·1357. (We might conceivably, in some circumstances, even reject the hypothesis that *any* probability could be assigned.) Hypotheses about probabilities are such that our rules of rejection for them must not be so final as for non-probabilistic hypotheses. (Though there are no *absolutely* final rules of rejection for those hypotheses which take the form of scientific theories.) We should not reject the hypothesis that the chance of 'heads' for a penny is $\frac{1}{2}$ just because when we try the penny we get five heads with five throws. This view, that statements about probabilities are hypotheses, is due to Braithwaite.[4] In effect Braithwaite recognises probability to be a theoretical, not an experimental, magnitude, and in this he seems to be right. You do not measure probability, but you do test the hypothesis that a certain numerical probability can be assigned to a property. This ties up with the fact that normally we do not talk of 'measuring' theoretical magnitudes. It is more natural to say that someone 'determined' a probability or the charge of an electron than that he measured it.

I have tried to distinguish various sorts of measurement and magnitude. We have fundamental magnitudes, defined magnitudes, derived magnitudes and theoretical magnitudes. We have also various hybrids between various of these. Corresponding to all these sorts of magnitudes we have various senses of 'measure'. And indeed, as was remarked in the last paragraph, the word 'measure' does not occur naturally at all in connection with theoretical magnitudes. There is one further sort of thing, however, which is commonly regarded as measurement, but which is such that, if it is regarded as a member of the family at all, it must be taken to be very distant relation indeed. This is the reading of calibrated instruments. A man who notices from the speedometer of his car that he is going at 25 m.p.h. is not making a measurement in any important sense of that word. The men who really measured were the men who calibrated the instrument. Instruments with pointer readings are not so much methods of measuring as technological devices to enable us to avoid measuring, when measuring is difficult. The simple question 'how are such instruments calibrated?' brings us back eventually to the question of fundamental measurement. When Eddington used to say that science was essentially concerned with pointer readings he never seemed to have asked himself the question of how his scales and dials were calibrated, and if he had asked himself this question he would surely not have believed his idealistic philosophy to be so consonant with the practice of science.[5]

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[4] *Scientific Explanation* (Ch 5), Cambridge University Press, 1953

Though I am here concerned with the philosophy of physics, the ideas of this article may have some useful applications to the philosophy of psychology. For psychologists frequently confess themselves to be worried about the nature of psychological measurement. They have frequently given in to theorists who have argued that psychological measurement is not really measurement at all, and that they were very naughty to have gone in for it, or else, more commonly, they have taken the attitude that, though psychological measurement is very naughty, nevertheless do it if they must. Few, however, seem to have had an easy conscience. As Sir F. C. Bartlett has said, 'When Theory whispers low "You can't", Practice replies "I must" '.[6] Where, then, do the sorts of measurement that we find in psychometrics come in? Are they of the sorts that we have described, or are they distant relatives of these, or are they perhaps illegitimate? My suggestion is that we should regard the magnitudes got from psychological testing procedures as theoretical magnitudes.

Consider I.Q. It is not a fundamental magnitude. For what physical or psychological operation corresponds to the addition of an I.Q. of 95 to one of 105? Furthermore, it is not a derived magnitude. For what are the fundamental magnitudes and what is the numerical law relating fundamental magnitudes from which it is derived? The suggestion may be made that I.Q. is not a magnitude at all, but only a scale (like hardness). This suggestion, however, is ruled out by the way psychologists treat I.Q. They treat it, and other quantities derived from test scores, as a magnitude, not just as an order, because they apply arithmetical operations, including addition, when they calculate correlation coefficients, moments about the mean, and so on. The only thing left seems to be to regard such magnitudes as theoretical magnitudes. That this view can be justified can be made plausible from a schematic example. Suppose a psychological test yields scores whose numerical values have a Gaussian distribution for the population. Suppose also that perfectly normal magnitudes like height and weight also yield a Gaussian distribution. We can then conjecture that the test scores correspond in a linear way to the value of some unknown magnitude. (Unknown at present.) Physiological research might enable us later on to identify it as, say, the number of neurons in a certain part of the cerebal cortex. Until, however, such an identification is made, we must regard the test scores as indicative not of an empirical, but of a theoretical magnitude. That we can play around with our scores as if they were indicative of values of a magnitude (e.g. by taking moments about the mean) depends on the acceptability of our hypothesis and this can be tested only by its fruitfulness.

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[5] See N. R. Campbell, 'The Errors of Sir Arthur Eddington', *Philosophy*, Vol. 6, 1931, pp. 180-192

[6] 'Subjective Judgments', *Nature*, Vol. 166, pp. 934-5, 1950.

I suggest that some of the troubles that psychologists have got into over the nature of psychological measurement may have been due to their operationalism, which makes them look for a direct operational definition of a magnitude and to overlook the possibility that many apparently empirical magnitudes may, on a closer investigation, turn out to be theoretical ones. Furthermore, without a plausible theoretical justification at the back of it a method of psychological measurement is no more measurement than is racing to the rainbow's end a game. This is not because of any definition of 'measurement' or 'game', but simply because of the logical incoherence or incompleteness of the procedure suggested. The successes of psychometrics, however, are such that it is probable that in very many cases psychological magnitudes are perfectly respectable and that they are theoretical magnitudes.

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ANTHROPOLOGY AFTER FREUD[1]

By J. A. Barnes

Throughout his life Freud took a lively interest in anthropology. One of his last publications, *Moses and monotheism* (1939), deals with an anthropological topic. It is fitting, therefore, that we should look at the discipline of anthropology and try to assess how it has been affected by Freud's teaching.

I

Anthropology has changed greatly since Freud first became acquainted with the work of Frazer and others at the turn of the century. Among the many writers and thinkers who have helped to bring about this change, Freud must be placed in the first rank. His influence can be seen in the kinds of inquiries that are made in the field, the problems that are tackled, and the concepts utilized in analysis. Yet it is a significant fact that many of those works like *Totem and Taboo* (1912-13), in which Freud specifically sets out to examine anthropological themes, are seldom quoted by anthropologists and are comparatively neglected in anthropological teaching. Nevertheless it is mainly through these works that Freud's views on anthropological matters have been brought to the notice of his followers and the general public. To explain this paradox, we have to distinguish beween the influence of Freud on anthropology and the influence of anthropology, as mediated through the writings of Freud, on psycho-analysis. For, while anthropology and psycho-analysis have had considerable impact on each other this impact has been, at least in the past, rather lop-sided. It is, I think, an index of the great profundity of Freud's general writings and of their thoroughgoing effect on anthropological interests and concepts that the special contributions made by Freud to the anthropological controversies of his day are no longer of central importance. The anthropological issues that were under debate during Freud's formative years have either been abandoned in favour of other interests or are now tackled quite differently. This shift of emphasis is in part due to Freud himself. Thus, for example, Freud was interested in the origins of totemism and set out his views on this subject at some length. I do not think that there is any anthropologist today who would subscribe to these views, and indeed we would do well to remember that Freud put them forward in a much more tentative way than he adopted when writing about events nearer home.[2] Yet totemism, without reference to its legendary origins, remains a topic of keen contemporary interest, and the mechanisms, such as projection, identification, internalization and the like, which anthropologists invoke in explanation of its various manifestations, are derived from Freud and his disciples.

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[1] Based on a paper read in Sydney on November 26th, 1956 at the Freud Centenary meeting of the Australasian Association of Psychiatrists, N S.W Branch.

At the beginning of the twentieth century anthropology was still deeply under the sway of evolutionary ideas derived from the work of Lyell and Darwin. Social evolution at its simplest became merely a search for origins. In England this line of development culminated in the work of Sir James Frazer, and it was by reading his *Totemism and Exogamy* (1910) that Freud's interest in the subject was sharpened. Today the quest for origins has been abandoned, at least in its old form, and anthropology is now more concerned with studying how orderly social relations are maintained between contemporary human beings loving and hating each other in an ambivalent way that would be quite out of place in Frazer's scheme of analysis. We are sometimes inclined to regret that, although Freud read Frazer's numerous works with great interest and insight, Frazer never felt it necessary to read a word of Freud. Yet perhaps in the long run anthropology has benefited from this failure to communicate. In effect it has meant that younger anthropologists have been able to turn to Freud not to study his contribution to ethnological controversies that have petered out, but to learn what he has to say about relationships between individual fathers and sons mothers and daughters, husbands and wives. It is these findings which they have been able to utilize in their own work.

Freud's contribution to anthropological thinking has probably been greatest in the field of kinship, and we can best see in what way he has influenced modern social anthropology if we look at some of the relationships found within the family in various societies. Two factors determine this approach. On the one hand, Freud was profoundly impressed by the way in which a child's relationship to its parents forms the core of its personality development, and by the manner in which powerful emotions of love and hate strive for mastery behind the facade of domestic bliss and childish innocence. On the other hand, the study of kinship has long been one of the central interests of anthropology, and an interest which it has sustained largely alone among the social sciences. Hence a close link between Freud's teaching and kinship studies is to be expected.

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[2] Cf *Totem and Taboo,* Standard ed , p 102, footnote 1

In the study of kinship, we are concerned with the way in which persons behave towards their parents, grandparents, siblings, spouses, cousins, affines and others. The pattern of relationships which prevails between persons related by descent or marriage varies widely from one society to another and is frequently markedly different from that found in our own culture. These variations have attracted the attention of many theorists, particularly since information about them began to become available in quantity at the beginning of the nineteenth century. However, the views advanced by nineteenth century writers were of necessity based mainly on speculation and fragmentary reports and not on detailed observation, and in consequence were of the nature of informed guesses, lacking verification. The same criticism can be levelled at all branches of nineteenth century anthropology, but whereas since the Renaissance and earlier there had been vigorous and informed discussion about such topics as the development of the State, the growth of economic activity and the nature of law, comparatively little had been written about the forms of the family. Even today, when contemporary political and economic systems are embarrassingly well documented, family life remains hidden in obscurity. Many of us, indeed, would argue that family life can flourish only in obscurity. Thus Maine, Bachofen, Morgan, McLennan and Tylor, the anthropological pioneers of the mid-nineteenth century, had few foundations on which to build their theories of kinship. Hence it is not surprising that kinship theories of the last century, with their preoccupation with group marriage and the ignorance of paternity, seem even further removed from modern anthropological analysis than is the case in the fields of primitive politics and economics. Yet these were the studies with which Freud was acquainted and which he tried to reinterpret in the light of clinical experience. Thus is comes about that, although Freud's lasting contribution to anthropology is certainly of greatest importance in the area of family relations in which he was most interested, Freud may not have expected the form it has taken.

II

Here there is space to mention only a few of the many topics that might be chosen to illustrate Freud's influence on anthropology and on the analysis of kinship systems. I shall try to demonstrate how his concept of ambivalence has been utilized in explaining certain customary behaviour between kinsmen, and how his ideas about symbols and about the significance of jokes have been adapted in sociological analysis.

A striking feature of many primitive societies is the requirement that persons who stand in a certain social relationship to one another shall avoid each other. Thus, for instance, a man may be enjoined never to be seen by his wife's mother, or a woman may be required to keep out of the way of her husband's father. A man may have to take care that he is never left alone with his sister. Sometimes these prohibitions are extended to include, for instance, not merely a man's real wife's mother, but also any woman who might properly become his wife's mother when he marries. The avoidance may apply to the possessions or attributes of the person avoided, as when a man is not allowed to sit on the sleeping mat used by his mother-in-law, or a woman has to walk round the outside of the cattle byre where her husband's father's cattle are kept. Occasionally the prohibition takes the form of a ban on performing the same role at the same time, as for instance when a woman is required to finish childbearing when her daughter starts to bear children.[3] Usages of this type have been reported from all over the world and are not associated with any one form of political organization. Rather, they seem to arise out of conflicts that are latent in every family and which have to be controlled in one way or another.

In most primitive societies a man and his wife are dependent on the coöperation of their kinsfolk and neighbours to a very much greater extent than they would be in our own society. Their social universe is comparatively small. There are few social services available to them other than the assistance provided by their relatives, and they cannot utilize the skills of the innumerable technicians and craftsmen on whom we rely. They call instead on their kinsfolk for help in securing their supply of food, in building homes, and in dealing with the crises of illness, birth, death, and the like. They may depend on their older relatives for land, livestock and other resources and for assistance in getting married. Coöperation of this kind is necessary, but it does not come about automatically. There is often resentment at the claims made by relatives on a person's time, energy and capital, and frequently there is a divergence of interest between the various groups of relatives who are expected to coöperate. Thus in many societies there is considerable tension between the social group in which a woman grows up and the group to which she goes as wife. Her own people may think that she is ill-treated and exploited by her husband's people, who on the other hand may suspect her of favouring her natal group and of deceiving her husband. In any event her loyalties are bound to be divided. One of the ways in which an open breach can be avoided is to reduce as much as possible the amount of contact between the two groups. Some contact is indeed often inevitable, but it may be possible so to arrange social life that certain key figures in the two groups never come face to face with each other. Thus, in societies where their direct contact is not essential, rules of avoidance may be found whereby a man and his mother-in-law are positively prevented from meeting. Many avoidance rules are concerned with the control of hostility between affines, but similar rules are utilized in other contexts. For instance, in many societies there is marked conflict between successive generations. In them we find rules of avoidance or rules to prevent members of adjacent generations from eating together, or openly competing with one another for positions of leadership.

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[3] Fortes, *Web of Kinship*, p 223.

Rules of avoidance between affines, and between members of successive generations, seem to arise out of the social processes carried on within the domestic family. The family is the unit within which children are procreated and brought up in the culture of their parents and neighbours. In virtually all societies, the cultural values that are incorporated in the conscience or super-ego of the child include the belief that incestuous sexual relationships within the family are wrong and that a mate should be sought outside it.

In our own society we are generally content to limit the definition of incest to actual sexual intercourse, but many primitive peoples extend the prohibition to include a number of acts which are regarded as symbolically equivalent to sexual union. This extension provides a way in which the existence of the ban on sexual intercourse can be publicly, formally and persistently demonstrated, whereas we not only limit the ban, but usually endeavour to hide its existence. The possible reasons for this different approach are not immediately relevant here.[4] However, in many primitive societies, because of this widened concept of incest, a man or woman has to be careful not to perform actions which are regarded as symbolically equivalent to one or other variety of incest. A second kind of extension is found where it is considered incestuous for a person to have sexual intercourse with a close affinal relative, in particular with a spouse's parent or a spouse's child by an earlier marriage. In a monogamous system like our own, any abhorrence we may feel specifically towards sexual intercourse between people related in this way tends to be masked by our blanket disapproval of all extra-marital sexual activity; but even with us ecclesiastical rules about the remarriage of widows show that sexual intercourse with certain affinal relatives falls into a specially prohibited category. In those societies where polygyny is approved, and where some kinds of extra-marital sexual activity are permitted, the extension of incest to include intercourse with some close affines is of greater practical significance.

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[4] Cf. Glückman, *Custom and Conflict*, pp 54-80.

In these societies the extension of incest beyond the nuclear family shows itself in two ways: prohibitions limit the choice of second wives, and prescriptions govern how a man (or woman) must behave towards his affines. The prohibitions on plural wives need not concern us here and I shall consider only the rules that lay down the proper way of behaving towards one's affines. Very commonly a man must avoid his wife's mother or must behave towards her with marked formality and deference Similar rules sometimes apply to a woman and her husband's father. Behaviour that contravenes these rules is characteristically regarded as shameful or sinful, and as automatically entailing some terrible misfortune for the offenders and, sometimes, their relatives. Sometimes offenders are liable to punishment at the hands of some centralized authority or organized group of kinsfolk. But whether or not a penalty is imposed from outside, as it were, there is almost always a sanction imposed from within. The individual believes that to behave in this way would be wrong or bad, irrespective of whether or not he is found out, or alternatively he believes that, through some supernatural or mystical agency, retribution will automatically and inevitably follow the breach of custom. Breaches of custom occur, but they remain breaches, not customs, and the sanctions that uphold the customs are in the main internal sanctions, deriving from the conscience of the individual and the metaphysical beliefs that he holds, rather than external sanctions operating on the wrongdoer only when his crime is publicly known.

The internal sanctions that ensure conformity to the customary rules regulating the behaviour of a man towards his mother, sister and daughter in sexual matters arise, according to Freud, from the Œdipus complex and the derived abhorrence of incest. The evidence from primitive societies suggests that rules of affinal avoidance are sanctioned in a similar way. In a few instances the link with incest is recognized explicitly. For example, among the Nyakyusa of southwestern Tanganyika a married woman must avoid her husband's father. She must never look at him. If he approaches along a path, she must flee from it. She must not mention his name. Above all, she must not go near his bed, even in his absence. These prohibitions are extended to other affinal relatives. Nyakyusa explain their custom of making boys build new villages away from the dwellings of their parents by saying that it would be impossible for a man to live in the same village as his father; for if he did so, his wife and his father would not be able to avoid one another without an intolerable amount of interference with normal domestic living. To explain why a woman must avoid her father-in-law, they quote a myth in which a chief became enamoured of the wife of one of his sons. It is said to be 'very bad' and 'very rude' for a woman to see her father-in-law because 'it is as if you saw him sleeping with your husband's mother'. If a woman does go near her father-in-law's bed, Nyakyusa believe that she will bear a thin and sickly child.[5]

The same reliance on the sanctions of the super-ego is found with many of the regulations controlling social relationships between kin. Thus these rules are different in genesis and operation from those rules of avoidance where the main stress is on the differentiation of one group or category from another. For instance, in present-day South Africa many of the customs and laws governing social intercourse between members of different ethnic groups, and their utilization of public facilities, rely mainly on external sanctions. Naturally there are many instances where rules of avoidance not only are subject to the organized sanctions of political machinery, but also derive some of their force from beliefs about mystically-mediated disasters. Many of the rules concerned with the pollution of one caste by another in Hindu India seem to have been of this kind. Even if the penalties for breach of a rule of avoidance are almost entirely external, the rule may well be internalized in the super-egos of those who keep the rule. All systems of social control rely to a great extent on the bulk of the population feeling that it is wrong to do certain things, without stopping to calculate the penalty.[6] Indeed, when a breach of law ceases to be thought of as wrong, as well as illegal, we often say: 'The law has broken down'. We have come to look on many minor regulations in this light; parking regulations are an obvious example. All the fearful apparatus of the law, fines, imprisonment and execution, are demonstrably inadequate to maintain order if the consciences of the community are not on its side.

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[5] Wilson, *Good Company*, pp. 82-86 and *Rituals of Kinship*, p. 216.

[6] Cf. Nadel, 'Social Control and Self-Regulation', *Social Forces*, 31 (1953), p. 270.

The salient fact that emerges from an examination of the avoidance rules between kin is that within the family there is conflict. This conflict takes different forms in different societies, and is regulated in a variety of ways, but the common aim is to prevent conflict from continually flaring up and leading to a breach of social relations so that orderly life becomes impossible. Furthermore, as a result of the process of upbringing carried on within the family, the adult arrives at maturity with these culturally-approved mechanisms firmly embedded in his conscience or super-ego. It is in this way, rather than by the application of external force, that the norms of family life are upheld. This is hardly surprising when we remember that in all societies, but in many primitive societies to a much greater extent than in our own, the cultural tradition is transmitted to the growing child largely by its parents and other close relatives, and not by some impersonal agency like a radio programme, books or a government department.

Thus, whereas earlier anthropologists tended to stress the solidarity of the kin group against outsiders, later writers, following Freud, have paid greater attention to the stresses and cleavages within the group of kindred and in particular within the elementary family. In certain contexts kin groups do display solidarity, and indeed a great part of recent work in the field of kinship consists of the analysis of these contexts and of the social relationships they involve. However, while groups of kinsfolk, like other groups, may present a united front to outsiders, they are also segmented internally. The internal cleavages not only characterize relationships within the group, but also influence the behaviour of its members towards persons outside it.

III

Although Freud's influence is apparent in the analysis of rules of avoidance, I think his specific contribution is shown even more clearly in the modern treatment of what are termed 'joking relationships'. In many societies a man is enjoined to behave towards certain relatives neither with deference and respect nor with avoidance, but by playing practical jokes.[7]

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[7] Radcliffe-Brown, *Structure and Function*, p 90

We have a very attenuated form of this practice in our own culture. With us, a man does not avert his eyes if his wife's mother comes into the room, for there is no mother-in-law avoidance. But in popular phantasy, as revealed by songs and by the gags of professional comedians, we do make jokes about mothers-in-law, particularly the mothers of wives, even though few of us do in fact crack jokes at the expense of, and in the presence of, our own mother-in-law. However, in many primitive societies, a man does tease his grandfather or his mother's brother or someone else standing in some specified relationship to him. Typically, a man behaves thus to his wife's brothers. Thus a Cewa man sitting on the verandah of a hut with his fellows may suddenly turn to his wife's brother and say 'Brother-in-law, I see a louse in your hair'. As the brother-in-law starts to search for the louse the other bursts into laughter, as much as to say 'April fool'. Possessions belonging to a brother-in-law may often be removed with impunity, and he may be teased and abused in public. To take offence at such treatment is to indicate that one wishes to terminate the relationship. The joking may be one-sided, or it may be reciprocal.

Two of the points that emerge in connexion with these joking relationships are relevant here. The first is that relationships of this kind seem to arise in situations where there is a great deal of tension and latent hostility between the partners, but where there are other considerations which rule out the possibility of controlling the conflict by a rule of avoidance. Thus among the Cewa, mentioned above, it would be difficult for a man to avoid his brother-in-law. Often they live close together, they are likely to be of approximately the same age, and they have to coöperate in economic and social tasks in the community.[8] There are thus in their relationship elements of common interest as well as of rivalry and division. In this situation of ambivalence, joking provides a means whereby the hostility existing between the partners can find symbolic expression without disrupting the relationship. Instead, the relationship is reinforced. It is acted out in public in a manner calculated to attract general attention. A man may feel resentful because his wife is being influenced by her brother, and he then gets rid of some of this resentment publicly by making a fool of his brother-in-law. At the same time he demonstrates publicly that he is indeed his brother-in-law and that they both have an interest in the well-being of his wife and her children.

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[8] Marwick, 'The Social Context of Cewa Witch Beliefs', *Africa*, 22 (1952) pp 128f.

The second interesting feature of many of these joking relationships is that in many societies they are specifically concerned with death. Indeed, some writers have described them not as joking relationships, but as 'funeral friendships'.[9] Another term used is 'cathartic alliance.'[10] In many societies the joking relatives are responsible for arranging certain details at a funeral. They may be called in to deal with the burial of those who are ritually unclean, like the victims of leprosy. In some societies they make jokes at the graveside, in a way reminiscent of the grave-digger in 'Hamlet'.[11] There is a wide range of situations in which joking relatives have tasks to perform, and it is difficult to generalize for them all, but on the whole it seems that one of the roles joking relatives commonly play is that of ensuring that social life is resumed after a serious realignment of relationships has become necessary, as for example after a death. They act, as it were, as an external coercive force in situations where the resources of the close group of kinsmen and neighbours are inadequate to cope with the situation. By their joking they provide a way in which the pent-up feelings of the kin of the deceased can find some relief. They do, of course, also provide social services in societies that lack specialized institutions such as undertakers, but that is not the point I wish to stress. What seems relevant is that in the presence of death, when the emotions of those closely connected are subjected to the greatest strain, and when a major adjustment has to be made in the social relationships of the group to which the deceased belonged, a number of persons come in from outside to provide support in getting the necessary tasks done, to relieve the mental tension, and to enable the bereaved to face every-day life again. The values of the society are reaffirmed and its continuity secured.

Joking relatives who appear at funerals are sometimes affinal kin, but they are sometimes recruited on some other basis, in terms of membership of a clan or age-set. A characteristic of joking relationships seems to be that they exist between persons, categories or groups that stand in some kind of ambivalent relationship to one another. Thus, as between relatives, joking relationships are maintained with those who are not so close that continuous coöperation is essential and yet not so distant that they can be avoided. Similarly, where there is a system of joking relationships between the tribes of a region, as in southern Tanganyika during the nineteenth century, joking relationships may be formed with those tribes who are not so close that permanent alliances are necessary and not so distant that permanent enmity is no disadvantage.[12] In all these contexts, joking enables hostility to be expressed symbolically. The ambivalence of affect is paralleled by the ambivalence of expression or action that constitutes the joke.[13]

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[9] Stefaniszyn, 'Funeral Friendships in Central Africa', *Africa*, 20 (1950), p 290.

[10] Griaule, 'L'alliance Cathartique', *Africa*, 18 (948), pp 256ff

[11] Colson, 'Clans and the Joking-Relationship' *Kroeber Anthropological Society Papers*, 8 & 9 (1953), pp 53f

IV

Another mechanism for the expression and release of social tension that has been scrutinized by anthropologists in recent years, and which is analogous to some of the clinical phenomena studied by Freud, is the reversal of roles in ceremonial. In primitive as well as in industrial societies, persons are organized for various purposes in hierarchical systems, with one person in authority over several subordinates and at the same time responsible to a superior of his own. There are often ceremonies forming part of the social life of these systems. One of the features which appears to distinguish many primitive societies from our own is that among them in the course of these ceremonies abuse is hurled at those in authority. Many actions normally prohibited are allowed, or even required, and in many instances people play roles strikingly different from those they follow in everyday life. Gluckman writes: '. . . a most striking feature of their organization is the way in which they openly express social tensions: women have to assert licence and dominance as against their formal subordination to men, princes have to behave to the king as if they covet the throne, and subjects openly state their resentment of authority.' He goes on to say: 'Hence I shall call them rituals of rebellion.'[14]

Many of these ceremonial reversals of ordinary behaviour involve social relationships of superordination and subordination. It is widely realized that in any hierarchical system those in the middle of a chain of command are perennially in a situation of conflict. In one sense, whatever they do they are bound to displease somebody. A well known example is the foreman in a factory. There is a vast literature on the difficulties of his position, pointing out how he is supposed to represent the interests of management to the workers as well as the interests of the workers to management. If he always does what his work force wants him to do, management tend to regard him as obstructionist and uncooperative; yet if he does what management want him to do, his workers tend to regard him as a turncoat and a traitor who has been got at by management. Conflicts of this kind are in a sense inevitable, though they certainly need not be disastrous. They are one instance of a general pattern of conflict between rulers and ruled which is found in all societies. The short-term interests of the two groups are likely to be different. However, in most conservative societies the rulers and ruled have a common interest in the maintenance of the existing order of society. Even rival princes in dispute about who should be monarch all wish to uphold the monarchy. Indeed, they must uphold the monarchy, since this is the political system within which they struggle for power. Under these conditions we find that from time to time the conflicts in the society may be acted out in symbolic form. One of the clearest instances of this mechanism of social catharsis, as it has been called, is in the annual ceremonies of the Swazi kingdom, in which the king is, in pantomime, attacked by his subjects, accused of incest with his sister, and generally abused.[15] Similarly, when a new village headman is installed among the Ngoni, he must sit motionless with bowed head while his villagers and neighbours abuse him.[16] In both these instances, for a while the ruled become the rulers, and the ruler becomes the ruled. Yet this is done within a formally defined ceremonial setting which has a definite beginning and a definite end, and which is manifestly not part of every-day life. An opportunity is provided for the ruled to express some of their antagonism against the ruler; but also both ruler and ruled can express their conviction that the ruler shall be ruler and shall rule. Both elements in the ambivalent relationship can make themselves felt, and the relationship as such is confirmed and reinforced. There are obvious similarities here with the procedures of psycho-drama, but it is important to remember that, in these ceremonial situations, the intention is to reaffirm and consolidate an existing state of affairs, not to alter it. Perhaps it is this fact that partly explains why the formalized expression of hostility is absent from most of our own ceremonial life. Where the hostility of one group toward another is based on a real and long-term division of interest, where it is revolutionary rather than rebellious, then the symbolic acting-out conflict is unlikely either to affirm the existing relationship or to bring about the changes one or other group desires. It is perhaps significant that those few instances of ceremonial role-reversal which are found in our own culture occur in situations where the hierarchy of authority is not only emphasized, but is also generally accepted as right and proper. Thus it is in the army that we find officers waiting on the men for one day in the year, and in the navy the youngest boy in the ship dressing up as the captain, the next youngest as the commander, and so on in reverse sequence.

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[12] Radcliffe-Brown, op cit, pp 94f

[13] Cf Freud, *Psychopathology of Everyday Life* and 'Jokes and their Relation to the Unconscious'.

[14] Glückman, *Rituals of Rebellion*, p 3

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[15] Gluckman, op cit, and *Custom and Conflict*, pp. 122-136

[16] Glückman *et al*, 'The Village Headman', *Africa*, 19 (1949), pp. 101-4.

Thus, in the modern analysis of ceremonial, as in the treatment of avoidances and joking relationships, we see that emphasis is placed on the social relationships that prevail between actors in various situations, and on the conflicts, as well as the similarities of interest, that arise out of these relationships. We see how attention is given to the way in which the values and premisses of society are incorporated into the thinking of those who grow up within it, so that continuity and stability are preserved over the generations. The formalized actions of ceremonial and the spontaneous ambiguity of joking are seen alike as the expression of ambivalent affect in symbolic form. Clearly anthropology after Freud utilizes many concepts analogous to if not identical with those he developed; it did not employ them sixty years ago.

V

In any scholarly discipline new ideas flow in from a variety of sources, and it is often difficult to attribute a change of approach to any one innovator. This is particularly true of anthropology, occupying as it does a central position between the arts and the sciences and thus exposed to winds from every quarter. Freud was not himself an anthropologist, and therefore it is difficult to say exactly how much modern anthropology owes to him. Yet I hope that I have shown that in their analysis of social phenomena anthropologists owe much to the stimulus of Freud's teaching. In his treatment of dreams he drew attention to the importance of personally-defined symbolism, and thus opened the way for a re-examination of the rich ethnographic material on culturally-defined symbolism. He emphasized how the experiences of childhood colour the dispositions of maturity, and thus encouraged others to look at human beings as the end-products not only of millenia of cultural evolution, but also of their own personal experiences. Attention was shifted from conjectured historical processes stretching forward from the earliest human origins to short-term processes that could be seen or easily recalled. But Freud's greatest contribution to anthropological thinking arose from his insistence on the family as a field of conflict. Even in his own ethnological speculations, Freud postulated a family in conflict, the sons of the horde who killed their father and felt guilty for having done so. Writers on kinship before Freud treated the family in terms of arranged marriages and inherited property, and were not particularly interested in rivalry between father and son, between older and young siblings, or between father-in-law and son-in-law. Freud demonstrated how these phenomena could be studied, and interest in his approach was stimulated among anthropologists by Malinowski,[17] who emphasized the authoritarian role of the mother's brother among the matrilineal people of the Trobriand Islands. Further examination of the sources of intra-familial conflict has led not only to a deeper insight into these tensions, but also to a better appreciation of the mechanisms whereby, on the whole families remain as units and do not burst apart.

At the beginning of this paper I drew a distinction between Freud's influence on anthropology and the influence of anthropology on psycho-analysis through Freud's papers on anthropological topics. When Freud published the first part of what was to become *Totem and taboo* he called his article 'Some points of agreement between the mental lives of savages and neurotics'. Anthropological field work during the last forty years has shown that savages and non-savages are much more alike than Freud may have supposed, but, together with the work of clinical analysts, it has also demonstrated that the phantasies and obsessions of neurotics throw into sharper focus tendencies present in all mankind, savage and non-savage alike. On the whole, I think that anthropology has gained more from psycho-analysis than psycho-analysis has gained from anthropology. Nineteenth-century anthropology has had a delayed impact on present-day psycho-analysis, but contemporary analysts have not, I think, taken the same lively interest in anthropological controversies as Freud took in his day. Far too little psycho-analytic work has been done among non-Western peoples and there are, particularly outside the United States, far too few instances of collaboration between members of the two disciplines. No doubt there are good reasons for this, but I believe that anthropology still has something to offer.

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[17] *Sex and Repression in Savage Society* (1927)

LAWS OF THOUGHT

By Leonard Goddard

When it is said that the laws of logic are necessary, it is implied that we must accept them. But why must we?

It is fashionable to answer this question in terms of linguistic conventions; one points to the meanings of the key words in them, words like 'and', 'not' and 'if . . . then'. A law such as the law of non-contradiction, 'not (p and not-p)', must be accepted simply because it follows from the definitions of the only two words in it, 'and' and 'not'.

Thus, whatever sentence 'p' we choose, if it is true then its negation, 'not-p', is false, and *vice versa;* and whatever two sentences 'p' and 'q' we choose, the compound sentence 'p and q' is true in case both components are true, false otherwise. These decisions (?) are recorded schematically in definitional truth tables,

p	not-p
T	F
F	T

p	and	q
T	T	T
F	F	T
T	F	F
F	F	F,

which, it is said, provide us with all the information we require in order to demonstrate the necessity of the law 'not (p and not-p)'. Thus, writing down all possible combinations of truth and falsity for 'p' (col. 1), we can then calculate the truth-value of the whole expression simply by reading off the appropriate values from the definitional tables. That is,

not	(p	and	not-p)
T	T	F	F
T	F	F	T
4	1	3	2

(the columns are numbered in the order in which they are calculated).

We see, therefore, that under all possible physical conditions, sentences of the form 'not (p and not-p)' must always be true (col. 4). For since 'p' may be replaced by any sentence, no matter what its meaning, and since all possibilities of truth and falsity are exhausted by the table (col. 1), both the meaning and the truth-value of any sentence which may be used to replace 'p' is irrelevant to the acceptability of the whole expression. Whatever sentence is used (whatever is the case in the world), 'not (p and not-p)' cannot fail. It is a truth which holds independently of the facts of life — as against, say, 'Stones fall to the ground when released from a height'; it holds solely by virtue of the definitions of 'not' and 'and'; it is a definitional, or verbal, truth. So, whereas, because the world might change, it is rational to doubt 'Stones fall to the ground . . . ', it would be irrational to doubt the law of non-contradiction. For *no matter what* changes take place in the world, it must always be true.

But suppose we press this a little and ask about these definitional truth tables. *Qua* definitions they are, after all, peculiar. One expects definitions to take the form, "the word 'not' means . . . "; but they are not like this. I might define the word 'bachelor' to mean 'unmarried man' and I should then be able to demonstrate from my definition that all bachelors are unmarried men. Here would be a definitional, verbal truth, in the ordinary sense. But is the law of non-contradiction like this?

No, it might be said, because the definition of 'not' as given by the truth table is implicit, not explicit like the definition of 'bachelor'. What is defined by the truth table is the *use* of 'not'. And with words like 'not', use exhausts meaning. An explicit definition, on the other hand, always in the last resort demands some non-linguistic knowledge before the meaning of the defined word is known. One might go on giving definitions of 'bachelor', 'unmarried', 'man', *etc.*, but in the end it would come down to marriage licences. Somewhere the dictionary has to be linked to the world, and the link must be known. Even if it is held that, with all words, meaning is use, still the meaning of a word like 'bachelor' is tied firmly to the variety of contexts in which it is (or may be) used; and linguistic contexts are not themselves linguistic, they are physical. The meaning of 'bachelor' is a resolution to use a certain sound or mark in a certain way, plus contextual references. The meaning of 'not', on the other hand, is just a resolution. Its use is context-free. And this simply strengthens the point that 'not (p and not-p)' is a verbal truth. What is meant by saying this is just that it follows immediately from implicit definitions which in themselves are neither more nor less than resolutions to use the words 'not' and 'and' in a given way; nothing more, no contextual reference, is required. Indeed the law of non-contradiction is itself an implicit definition of 'not' and 'and'. Taken along with all the other laws containing these words, it defines their interrelated use. Hence, there is a difference between the law of non-contradiction and the truth 'All bachelors are unmarried males', but it is a difference which operates in favour of the thesis that the law is entirely verbal in character.

But yet, one wants to reply, whose resolutions are these? And why *this* use (as given by the truth tables) rather than another? As a matter of fact, the word 'not' often is used in ways which simply do not square with the definitional table — go to any kindergarten! Is the definitional truth-table simply a summary of majority usage? If so, then, as linguistic habits change, the laws which follow from these definitions will also change. If definitional truth tables are generalizations based on the linguistic habits of particular sections of the community, then, in an important sense, logical laws are inductive laws descriptive of human behaviour. What happens in the world is relevant to their acceptability; for people may decide to talk in different ways. A last-calculated column of T's in a demonstrational truth table simply records that, as a matter of fact, the majority of people here and now use the words 'not', 'and' and so on, in such a way that, say, 'not (p and not-p)' is always true. Hence, though the behaviour of stones will not affect the law, the behaviour of people will; a change in linguistic habits will imply a change in the law. Where now is the 'must'?

But there is more to it than his. The laws of logic do not record (describe) human behaviour in the way in which the laws of psychology do; and they are not, as the laws of psychology are (or ought to be), held ready to be modified as human behaviour changes or as more information becomes available. For in logic we are dealing with resolutions, not with the way in which people do as a matter of fact talk but the way in which they have resolved to talk. Whose resolutions? The majority's, perhaps, though this is not important. Why this use rather than another? Because it is the *conventional* use. And this is where the 'must' comes in. For the resolutions, the definitions, are enforced. The child in the kindergarten who uses the word 'not' in an unusual way is told that he is wrong to do so; quite certainly his behaviour is not used as evidence against the law of non-contradiction. And there are sanctions in later life too. Social praise and blame, for example (as in the use of loaded words like fallacious' 'muddled', 'circular'; or, on the praise side, 'valid', 'clear-thinking', 'logical'); and in the last resort, the lunatic asylum. At the fallacious-muddled-circular end, the 'must' is the gamester's 'must': you must move the rook in a straight line. At the other end, the lunatic-asylum end, the 'must' is the policeman's "or-else" 'must': you must wear clothes in public. Play the (linguistic) game as the community plays it, or else! And of course there are a number of other 'musts' in between, from the mild socio-conventional (you must be nice to Mrs. Jones) to the stronger political (you must take your shoes off when you go into the temple).

But if the laws are conventional, they can be broken. Of course: people do argue fallaciously! Yet not only can they as the result of being ignorant, and making a mistake, but as the result of a deliberate decision. If I *choose* to talk differently, being entirely aware of the existing rules of the game, I should not be committing fallacies; I should simply be playing a different game. I might incur the wrath of the community, run the risk of not being understood, but that is all. My only problem, it seems, if I want to argue differently *yet logically,* is to persuade others to adopt with me a new set of conventions, a new set of logical laws. In this way I could set up my own linguistic community; and what is fallacious in the original community may not be fallacious in mine. For me, it may be, the law of non-contradiction would not be a law. It would fail *significantly.*

No doubt it would be difficult to do this, but, if laws are conventions, there is nothing impossible about it. It would be difficult; first, because that vast network of logical and grammatical laws and rules which serves as the *status quo* of discourse is so instilled — from the knuckle-rapping at school to the 'muddled'-sneers of later life — that it is taken for granted in all discourse; and secondly, because linguistic habits, being habits, are difficult to change But not impossible; for conventions are alterable at will and by persuasion.

So we *can* talk (think, argue) differently, in the sense that it is possible. But we ought not to. This, finally, is the heart of the 'must'. And we ought not to for exactly those reasons that rebels always face. Upsetting the *status quo* always incurs social and moral reprobation

This kind of view is not uncommon, and it has been taken up by C. A. Campbell in a recent article.[1] He takes as his text a quotation from Ayer:[2]

> "In the course of the debate Ayer agrees that one can't admit the possibility of a piece of paper's being white and not-white at the same time 'given existing conditions about the use of the word "not".' He goes on, 'But of course you could prefectly well introduce a convention by which it would be meaningful. Suppose you chose, when the paper was grey, to say it was white and not-white. Then you would have altered your logic. But given that you're using a logic in which you exclude "p and not-p", then of course you exclude the paper's being white and not-white'."

Campbell argues, it seems to me convincingly, against this view and in favour of the view that laws of logic are laws of thought in the old-fashioned sense (*i.e.* that they are necessary conditions of intelligible discourse). What he is concerned to show is that a convention such as

'This is white and not-white' is to mean 'This is grey' leaves the problem untouched since the *law of non-contradiction* has not been denied or changed; what has been changed is only the English language. To make good the view that laws of logic are conventional laws, it has to be shown that "it *is* possible to assert a self-contradiction intelligibly, if you alter your logic".

I want to add a few remarks by way of support on Campbell's side, and then suggest that nevertheless the laws of logic might yet be conventional in one sense (though perhaps an extended sense) of 'conventional'.

Logical laws are not *just* verbal conventions; at all events, not conventions like: the word 'different' should be followed by 'from'. They are necessary conditions of intelligible discourse in the sense that if I say "Stand up and do not stand up", my audience will experience a sense of frustration. They will not know what to do because they will not understand me, whereas they would understand me if I said, "This is different to that" — though they might frown. When I say, "Stand up and do not stand up", my words stifle action (except that the audience will laugh at me) unless, of course, I have already briefed them to stand on their heads when

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[1] "Contradiction: 'Law' or 'Convention'?", *Analysis*, 18 4, pp 73-76

[2] In *A Modern Introduction to Philosophy*, edited by Paul Edwards and Artur Pap, pp. 586-618.

I utter the 'command'. But in this case I should simply be using an unusual form of words to say, "Stand on your heads"; I should not be saying "Stand up and do not stand up"; I should not be uttering a significant yet contradictory order. Again, I often do say, "It is and it isn't", meaning by this, 'partly so and partly not so'; that is, I mean to assert that the alternatives put up to me are not exhaustive. Here again, however, I am not making a significant *contradictory* statement. On the contrary I am saying that there is no contradiction involved in accepting both alternatives.

The dilemma is this: if logical laws are merely linguistic conventions, then a change (deliberate or otherwise) in our verbal habits ought automatically to entail a change in our logical habits. That is, if we change the meaning of 'not', argument forms which hitherto were valid ought no longer to be so, and those which were invalid ought now to be counted as valid. But this simply does not happen. If we adopted, say, Ayer's convention, then we should talk differently but argue and think as before, introducing a new word for 'not'. We are not changing our *logic* if the meaning of 'not' is changed in such a way that 'p and not-p' has a non-contradictory translation. On the other hand, if it does not have a non-contradictory translation, then we are not changing the meaning of 'not'. Logical laws are conventions, perhaps, but not verbal ones. Indeed, to show that they are conventions we have to show that we can on occasion assert 'p and not-p' while yet retaining that meaning (use) of 'not' which we ordinarily accept.

The point is the familiar point often made about arithmetic. Of course two plus three equals ten if 'two' means what we ordinarily mean by 'seven'. But can we have two plus three equalling ten if 'two', 'three' and 'ten' mean what they now mean? Can we count differently, not can we talk differently?[1] Analogously, the problem here is not, can we talk differently? but, can we think differently?

Yet if there is anything in this plea, it ought to be possible to point to some flaw in the argument which purports to establish the purely verbal character of the law of non-contradiction; and indeed there are defects. Thus consider again the demonstration of 'not (p and not-p)'. We are asked to accept:

(1) that the law follows immediately from the definitions of 'not' and 'and', and from these definitions alone;

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[1] Cf Gasking, "Mathematics and the World" this *Journal*, Vol XVIII, 1940, pp. 97-116

(2) that these definitions adequately determine the use of 'not' and 'and' and therefore their meaning, since use exhausts meaning in such cases;

(3) that the acceptability of the law is therefore independent of the facts of life (excepting the linguistic facts of life), and hence that it is a definitional, or verbal, truth.

But both (1) and (2) are suspect.

Thus consider the first point, that the law of non-contradiction follows immediately from the definitional tables. It does, provided we first accept the law of non-contradiction. For if we are to accept the tables, and accept also the conclusion (3) which is drawn from the demonstration (that *no matter what* happens in the world, the law is always true), then we must accept that the rows of the table do indeed exhaust all physical possibilities. That is, we must accept that whatever sentence 'p' is chosen, there are only two possibilities for it: either it is true or it is false. But to say this is just to deny the possibility of any sentence being both true and false.

What is left out of the table is just as important as what is put in; and what is left out, provided it is agreed that it should be left out, amounts to an acceptance of both the law of non-contradiction and the law of excluded middle. For that we forbid rows of the sort,

p	not-p
TF	

which embrace the the possibility of p's being both true and false, and rows of the sort,

p	not-p
M	

(M = some middle value between T and F), which embrace the possibility of p's being neither true nor false, means exactly that the laws of non-contradiction and excluded middle are being taken for granted. These laws, that is to say, are essential presuppositions of the definitional tables, indeed of all the tables.

The demonstration, then, is circular. The law of non-contradiction does not follow from the definitions alone, it follows from these plus the laws of non-contradiction and excluded middle. Hence, even if the definitions were unexceptionable, the conclusion (3) would not follow. Before we can accept (3), we must accept the definitions and we must accept that the definitions imply the law. But the implication is suspect. So, too, are the definitions.

Thus, so far as (2) is concerned, the definitional tables adequately determine the use of the words 'and' and 'not' *provided* we already know how to use them. The schematic tables obscure this, but if we write the table for, say, 'and', in full, it immediately becomes apparent. For what we in fact have is,

if 'p' is true *and* 'q' is true then 'p and q' is true (1st row)
if 'p' is false *and* 'q' is true then 'p and q' is false (2nd row)
if 'p' is true *and* 'q' is false then 'p and q' is false (3rd row)
if 'p' is false *and* 'q' is false then 'p and q' is false (4th row)

Clearly, if we are to understand these hypotheticals, we must know what 'and' means; that is, we must know how to use it. And though one tried to gloss this by pointing to the fact that the italicised 'and' is strictly speaking a different word from that 'and' which occurs in the quoted expression 'p and q', since it occurs in a different level of language, still it has the *same use,* the conjunctive use. No-one, that is to say, could be taught the use of the word 'and' simply by presenting him with the definitional truth table. He would have to know this use before he could understand the table.

The table for 'and', then, is not a *definitional* table, because it is circular. And similarly, the table for 'not' is circular: For 'false' *means* 'not true'. That is the rows are,

if 'p' is true then 'not-p' is *not* true
if 'p' is *not* true then 'not-p' is true.

The reasons for saying that the law of non-contradiction is solely a verbal truth, therefore, seem not to be good ones. Rather is it that the law is unavoidable in the sense that it seems to be presupposed in all discussions about it. And if this is so, then it is not simply a *verbal* convention; instead, it is a necessary condition of intelligible discourse, a law of thought. Much more than social pressure is involved if we consider changing it. For to deny it significantly is not just to introduce a new meaning for 'not' (or 'and'); to deny it is to show that, given the existing meaning of 'not', a contradiction is significant. I now want to suggest that perhaps this is possible.

I take it, first, that it would be generally accepted that the principle of contradiction *qua* law of thought, *i.e. qua* necessary condition of intelligible discourse, involves a spatio-temporal reference. For we can say, taking the ordinary meaning of 'not', that the same point of an object is white at one time yet not white at another, and we can say that different points of the same object are white and not-white at the same time. That is, strictly stated, the principle is:

Not both p and not-p at the same point and the same time, where 'p' stands for any statement which affirms a quality (*e.g.* 'X is white'). Or, if 'p' happens to be a statement which itself refers to place or time (*e.g.* 'X is here'), then the spatio-temporal rider is dropped or modified accordingly.

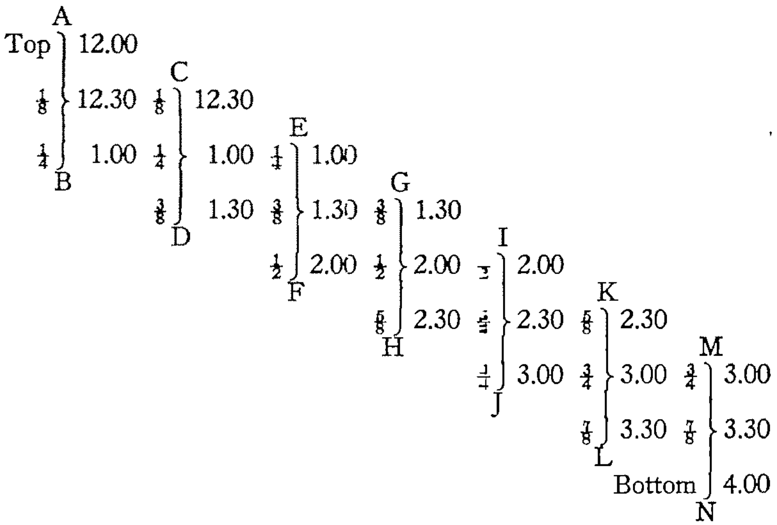
This being so, it may well be that if we change our conventions concerning space and time (whatever these are and supposing it possible for the moment) then the necessity of the law will be lost though we retain the ordinary meaning of 'not'. Given the ordinary meaning of 'not' and the usual space-time order, 'not both p and not-p' is a law of thought; we cannot think intelligibly except in terms of it. But, given the ordinary meaning of 'not' and a different space-time order, then, it may be, 'not both p and not-p' is not a law of thought; *i.e.* cases of 'p and not-p' are significant.

In order to illustrate this, I want to imagine that universe we all dream of, a universe in which it is possible to 'put back the clock'. Only, in the universe I am thinking of, not only is this possible, but things just happen that way. Natural processes repeat themselves according to a fixed pattern which is mirrored in clocks that work in the following way:

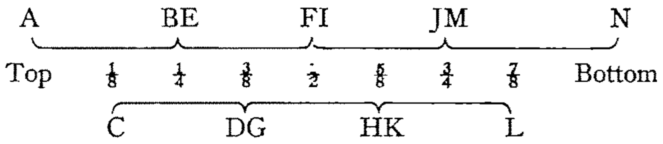
Starting at twelve o'clock, say, the minute and hour hands move in the usual way until they record one o'clock. They then continue from half-past twelve and move continuously through to half-past one. They then move from one o'clock to two; then from half-past one to half-past two; and so on.

This way of recording time is forced on the inhabitants by the ordinary pattern of events. A stone which rolls down a hill during the first occurrence of a given half-hour, will again roll down the hill in exactly the same manner during the second occurrence of that half-hour. Everything happens twice.

In general, therefore, we could simulate the measurement of time in this universe in terms of our own system of measuring time by using two continuous time-scales, one lagging a half-hour behind the other. Thus, suppose that the stone begins its journey down the hill at 12 noon, that it is $\frac{1}{8}$ of the way down at 12.30, and $\frac{1}{4}$ of the way down at 1 p.m. At the second occurrence of 12.30 it is $\frac{1}{8}$ of the way down, at 1 p.m. $\frac{1}{4}$ of the way down, and $\frac{3}{8}$ of the way down at 1.30. It continues in this regular way until it reaches the bottom. The pattern is as follows:



The two continuous scales are therefore made up in the following way:



In order to picture the techniques of such measurement, we could run two of our (continuous) clocks, one lagging a half-hour behind the other, each linked to a buzzer which sounds at the appropriate times (*e.g.* when clock 1 reads 1 p.m. and clock 2 reads 12.30) warning us to change from reading one clock to reading the other.

Within any one time-scale, therefore, since each is continuous and events happen consecutively, 'not (p and not-p)' will always hold; and even when both scales are taken into account still this will be true, since an event which occurs at, say, 12.45 the first time, is repeated exactly at 12.45 the second time. So no object is both white and not white at the same time either within one scale or in the extended sense of 'same time' which takes both scales into account. But the discontinuous moments are different.

For in this universe, space, or rather movement in space, as well as time, is discontinuous. The boulder which begins its journey down the hill at 12 noon and is $\frac{1}{8}$ of the way down at 12.30 and $\frac{1}{4}$ of the way down at 1 p.m., is back at the point $\frac{1}{8}$ of the way down at the second occurrence of 12.30. And this discontinuous change of position takes no time, otherwise neither space nor time would be discontinuous — the boulder would move so quickly as to defeat the eye perhaps, but still it would move through time, *i.e.* it would move *quickly,* and hence time could be measured on *one* continuous scale. Yet if no time elapses while the stone moves from B to C then at that moment it is in both places: *i.e.* it is at B and it is not at B.

At the moment before the discontinuous change the stone is approaching B, at the moment after the discontinuous change it is leaving C; at the moment of change, therefore, it is at both B and C. For from the continuity of the first scale it must reach B, and from the continuity of the second scale it must have been at C.

The principle of non-contradiction fails, therefore, in the form in which 'p' itself refers to position ('This is here and not here at the same time'). But it also fails in the form in which 'p' makes reference to a quality. If it happens to be raining at 1 p.m. but not at 12.30, then at the moment at which the stone is at both B and C each point of it is both wet and not wet.

Hence, although 'not both p and not-p' is a law of thought in the sense that it is a necessary condition of intelligible thinking about any moment other than the discontinuous ones, so also is 'p and not-p' a law of thought *qua* condition of intelligible thinking about the discontinuous moments, if movement happens to be taking place on both sides of them.

Campbell says, " . . . it would seem that, in logic, we are *obliged* to accept the rule 'p excludes not-p'; and if we are obliged to accept it, it is not a *convention.* It is a necessary rule for intelligible discourse — 'necessary', because we are just not able to think save in terms of it. But this is really equivalent to saying that it is a 'law of thought'."

I have wanted to show that we can accept the force of this in the sense of 'convention' in which Campbell intends it, but that the obligation on which he rightly insists, though it implies that the rule is a law, nevertheless does not exclude the *law* from being a convention in a wider sense. That is, 'not (p and not-p)' is both a law of thought and a convention. For since we can imagine a universe in which it fails, is seems clear that our ways of thinking, hence our laws of thought, are determined by the spatio-temporal conditions in which we live. Things happen in a certain way and this forces us to think in a certain way if we are to think intelligibly about those happenings. We are *obliged* to accept the rule 'p excludes not-p' because our space and our time are continuous, but, had the universe been different, so also might the obligation have been different, and so also might the laws of thought.

Yes, it will be said, our ways of thinking are determined by the spatio-temporal conditions in which we *live,* but not by spatio-temporal conditions which we *lay down.* Things just are so, and this causes us to think so. It has not been shown that, within our universe as it is, the laws of thought are conventional, because it has not been shown that the spatio-temporal conditions are conventional. If anything has been shown, it is, in an extended sense, Kant's point: that the necessity of our laws of logic and mathematics, hence the possibility of intelligible thinking, is firmly wedded to a space-time order. Only he wanted to say in addition that, for us, there could only be one such order. And this is the real issue. Is our space-time order a *conventional* order in the same sense that it is alterable in such a way that we still can control the happenings in the universe, that we still have a useful physics, geometry and logic? If so, then we can accept that, though we are obliged to think in a certain way if we are to think intelligibly, still the laws governing this intelligible thinking are conventional because the order in which they determine conditions of significance is itself conventional. But this has not been shown.

And in order to settle it, it would seem, it would have to be shown that we could look at the happenings in our world through different spectacles and "see" it as a discontinuous world; it would have to be shown that our ordinary way of perceiving is a conventional way, only one among many possible successful ways. The foregoing discussion is not evidence one way or the other so far as problems of this sort are concerned. For that a discontinuous universe is to some extent describable, and that we can understand that the inhabitants of it would have to think differently from us if they think intelligibly, is not by itself evidence that *we* could think and talk intelligibly within our universe in this fundamentally different way. Describing, recognising, understanding, thinking *about* the possibility of such a universe, is not the same as thinking *within* an actual universe of this sort. If we are to show that our space-time order is a conventional order, we must first change our ways of perceiving, hence our ways of thinking, hence our physics, geometry and logic.

Nor is the creation and use of non-Euclidean geometry a step towards this. Finite or infinite, linear or curvilinear, these are not the right kind of alternatives. Non-Euclidean space is not fundamentally different from Euclidean space in the way in which it would have to be to make good this point (or to constitute a refutation of Kant). Motions in it are still continuous, bodies are extended, and positions are distinct and uniquely describable. To make the point, or to answer Kant, a geometry which fails to satisfy conditions of this sort must be both consistent (in terms of the new logic) and usable.

And yet, suppose that there are, in the discontinuous universe, observers who are not themselves subject to the discontinuous changes but who "sleep", without being aware of it, in one-hour stretches, separated by one-hour stretches of wakefulness, in such a way that they miss all the discontinuities. Thus, these privileged observers are awake from 12 noon until 1 p.m. while the stone moves from A to B. Automatically, they then "fall asleep" for the period from 12.30 to 1.30, while the stone moves from C to D, and awaken at 1 p.m. (its third occurrence). They then stay awake until 2 p.m. while the stone moves from E (*i.e.* B) to F, when they automatically "fall asleep" again. Time and motion for these observers are continuous. For them, the stone rolls continuously down the hill from top to bottom since they only live (are "awake") within the first continuous time-scale from A to N. These people could not perceive differently, perhaps, but they could think differently if someone told them what the universe is really like. And they could alter their physics, geometry and logic accordingly.

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MODAL EXPRESSIONS IN ORDINARY AND TECHNICAL LANGUAGE

By R. Harre

The study of modal logic ought to begin, though it often does not, with a preliminary study of the way the key expressions "necessary", "possible", "obliged", "must", "may" and so on are actually used. There is a tendency for logicians to rush into algebraic representation without any very clear idea what it is they are representing; with a consequent confusion of interpretations when the meanings of formulæ come to be considered. This unhappy state of affairs is usually, though not always, produced by there being too few distinctions made, too few use-families noticed. If some such preliminary study as I shall make here is used as a basis for representation and the creation of formulæ, purely linguistic difficulties are not so likely to upset the algebras later on. In such a discussion as this it is clearly impossible to cover the whole field of use of modal expressions and I shall confine myself to trying to make clear some central cases only.

It is not unreasonable to suppose that language was first used for doing rather than for saying, used to bring desired and desirable consequences about rather than to convey information. Language, it has been said, is fundamentally *performative*. Of performative uses of language surely the most fundamental is the command and, linked with it, the expression of obligation. Commands can be expressed by mood but the sense of compulsion they convey can also be carried by the modal auxiliary "must". "Go!" expresses a command in the imperative mood; "You must go" expresses, by the use of the modal auxiliary, both a command and the obligation upon you to go. (Consider the use of "You must go" as an *explanation* of the command.) The expression "must" can be used to turn an informative statement, "He goes" into the performative "He must go". It is important to notice that "He must go" is as much a performative as "You must go", for though it is not itself a command it may initiate the process that ends in a command by expressing a resolution. A board of directors decides to dismiss an employee in the words "He must go' and this decision is conveyed to the man in the command "Go!" The notion of obligation imported here by the use of "must" is perhaps the fundamental notion in the galaxy of concepts it is my purpose to elucidate.

The different modes or ways of making assertions is one of the features of language that the expressions of obligation have been used to mark. It is these that have mostly interested logicians and we must try to see what could be the point of using this particular notion of obligation in what seems a very remote context of use. Though the discussion of modes of statement-making goes back to Aristotle, we can quite profitably begin with the classification used by Kant.[1] He thought that there were three kinds or modes of assertion — the problematic, the assertoric and the apodeictic. The representation of these ways of asserting is one use of modal expressions. It is from this use that the current logical term "modal" comes. In English a representation of these modes is, for example:

Problematic	I	It may be red.
Assertoric	I	It is red.
Apodeictic	I	It must be red.

With the help of the word "true" we can express another similar gradation of assertion without interfering with the internal structure of the sentence modified. In the following examples the modal auxiliary is used not to modify the basic locution but to modify the ascription of truth.

Problematic	II	It may be true that it is red.
Assertoric	II	It is true that it is red.
Apodeictic	II	It must be true that it is red.

There is a strong temptation to assimilate these constructions one to another and to other constructions built in a similar way out of the apparently corresponding Romance expressions "necessary", "possible", etc. For example:

Problematic	III	It is possible that it is red.
Assertoric	III	It is the case that it is red.
Apodeictic	III	It is necessary that it is red.

and

Problematic	IV	Possibly it is red.
Assertoric	IV	Actually it is red.
Apodeictic	IV	Necessarily it is red.

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[1] *The Critique of Pure Reason* abridged N Kemp Smith, p. 74-75.

We shall have made a major advance in the study of modal logic if we can be brought to see that there are four different cases here.

That there is something unhappy about assimilating these sets to one another, tacitly carried through by the use of one kind of modal symbol for them all, is clear. Only through the problematic does there seem, even *prima facie,* to be a thread of genuine synonymity. Further, Apodeictic III is grammatically incorrect as it stands, for "it is necessary that . . . " takes the subjunctive in the dependent clause. There is evidently a case for more detailed investigation.

The basic modal notions of "command" and its complement "obligation" were expressed above by the use of "must", but they also find expression in the use of "necessary" and its cognates in various constructions. The analysis of these uses will be aimed at expressing what they convey in terms of obligation. I suggest that by analogy with persons being obliged to act in various ways we arrive at the notion of statements obliged to be true and events obliged to occur. The analogy goes by a steady gradation through obligations of various kinds on us to judge certain statements true, to bring about certain events, to a kind of logical and epistemological anthropomorphism by which we feel the statement to be obliged and the event to be constrained. We shall understand the use of "necessary" in these contexts when we have uncovered the source of the "obligation".

I suggest as a preliminary analysis of "Necessarily *p*" (in one sense) and of "*p* is (logically) necessary", "*p* must be true". This last can be paraphrased for the purposes of the investigation as "*p* is obliged to be true". What is the source of the obligation?

There are sentences which, whenever they are used, are used to make a true statement, and for which the truth of the statement made depends upon grammar and not upon context or content. A sentence *framed* in a certain way, we may say, *cannot but be* used to make a true statement; and statements made with this sentence *cannot but be* true. Here is a source of "obligation", for, given the grammatical structure, no alternative to a judgement of truth for statements employing this structure can be envisaged. The class of tautologies will be necessary in this sense; so that the precise sense "*p* is a tautology" can be given for "Necessarily *p*".

Another class of sentences which cannot be used to make false statements are those depending upon semantic generalities, that is those in which the meaning of the words and the grammar of the sentence combine to ensure the truth of any statement made with it. This group of sentences can also make some claim to give rise to statements "obliged" to be true, for, since their truth does not depend in any way upon contingent facts, they *cannot but be* true. This group of sentences and the statements made with them are those commonly called *analytic.* The class of analytic locutions will be necessary in the required sense; so that another precise sense, "p is analytic", can be given for "Necessarily p". It is usually held that the class of analytic statements includes the class of tautologies.

An interesting subsidiary case is that in which we call the conclusion of an argument "necessary" or say that the argument itself is necessary, or sometimes that the conclusion follows necessarily. Here we are marking the obligation upon us and everyone else to draw just *this* conclusion in a certain kind of argument.

The obligation to action finds expression in the use of "It is necessary that . . . " followed by a dependent clause in the subjunctive describing the action. There is also, with this construction, a strong feeling that the action described in the dependent clause is a condition for the attaining of some further end. "It is necessary that these buildings should be demolished" is quite unsatisfactory to an occupant as a *reason* for moving him out, since it is incomplete. He can ask "Necessary for what?" and legitimately expect an answer. The force of "necessary" here is still that of marking an "obligation", for it is used to indicate that, the ultimate end admitted or agreed upon, what follows in the dependent clause is an *indispensable* condition for the attaining of that end. The "obligation" is created by our acceptance of the aim or end prior to our grasping the indispensability of the condition, an indispensability which may be shown by the quoting of a law. If we want to attain the end we are, so to speak, *obliged* to accept the condition. A paraphrase into the simple "must" idiom works well enough in this case, for "These buildings must be demolished" can quite correctly be followed by "What for?", to which an answer in terms of some aim seems genuinely to be required. However, in certain circumstances the "must" construction could be a polite form of command, say in giving their instructions to a demolition team. "These buildings must be demolished" may be a polite form of "Demolish these buildings!"

"Possible" has a more complicated pattern of use than "necessary", for constructions in which it occurs can be synonymous with "may", "might" or "can" constructions, the uses of which are very different. The essential performative notion behind these uses is "permission". Permission is connected in a complex way with command and obligation. For an understanding of the notion of permission we need to introduce the idea of a negative command, a prohibition. A common expression of prohibition is to use the word "forbidden", as for example in "Smoking forbidden". However, an explicit prohibition expression need not be used. It is quite common to express prohibition with an emphatic negative—"No smoking", "No entry", etc.

It would be too simple an analysis to state an equivalence between "permitted" and "not forbidden", for actions which are quite indifferent are not forbidden either. There is a positive though weak sense of approval about most grantings of permission that is not conveyed by the simple negation of a prohibition. This sense of approval is sometimes quite strong but can be almost non-existent. However, whether this sense of approval is present or not, what is important for logic is that the granting of permission makes an action possible, in the sense that it becomes one of our alternatives, that is it becomes an action the doing of which without penalty we can now contemplate. From this central notion the concept of permission runs out into statements and events which are independent of actions, in much the same way as we have seen the concepts of command and obligation develop senses that through various analogies apply much more widely than in the context of human action.

If I am right in supposing the uses of "possible" and its cognates to be connected with the notion of permission, then we can look for the various uses of "possible" by a method parallel to that used in the discussion of "necessary". We can consider the two notions "permitted to occur" (the idea of permission applied anthropomorphically to events) and "permitted to be true" (the idea of permission applied anthropomorphically to statements).

"Possibly *p*", "It is possible that *p*" and "*p* is possible" are all ways of saying that *p* might be true, that *p* is "permitted" to be true. To say that *p* might be true is to say that now, at this moment, *p* is not certain. One way in which a statement may be less than certain is when it is neither tautologous nor analytic, and there is an important use of "possible", in the phrase "logically possible", that is often taken to have this sense. As we do not think that self-contradictory statements might be true the full analysis of "possible" in this sense must be "neither tautologous nor analytic nor self-contradictory". But the relation between "It is possible that . . . " and "It is true that . . . " is not quite so simple. In the discussion above it was assumed that "possible" is used not as an alternative to "true" but rather to modify a bald assertion of truth. It should be noticed that when "It is true that . . . " means "It is certain that . . . " and "It is possible that . . . " means "It is uncertain that . . . " then "It is possible that . . . " is exclusive of "It is true that . . . ". This is clear enough when we use the certain/uncertain idiom but it can be and has been confused when we do not use this idiom — for there is a temptation to assimilate all uses of "possible" to "logically possible", where "It is true that . . . " is not in conflict with "It is possible that . . . ". However, if we take "*p* is possible" to mean "*p* can be true", then in one sense of "*p* can be true" we are saying that *p* is the sort of statement which is verifiable, that is to say we are classifying '*p*' as a contingent statement or perhaps as a statement of a certain logical form, a form not such as to predetermine its falsity (as it would if *p* were contravalid in form). These uses are all cases where "possible" is taken to refer to the statement and not to what the statement is about.

The idea of events being permitted to occur, or not obliged not to occur, leads through a similar anthropomorphism to our calling an event or situation "possible" either because there *is* no reason why it shouldn't occur or because we know of no reason. In this use constructions with "possible" are equivalent to constructions with "can". "It is possible to climb Mt. Everest" is equivalent to "Mt. Everest can be climbed". Many people and most children are muddled about possibility in this sense and ordinary permission, and this shows itself in the general uncertainty about when to use "can" and when to use "may". This sense of "possible" can be paraphrased into "compatible with what we know" or sometimes "compatible with how things actually are".

It is important to see that "impossible" fits in with some of these uses of "possible" and not with others. For example, "impossible" does not seem to fit in with "possible" when the latter has the sense of "less than certain", for the opposite of "certain" is "uncertain", for which we use "possible". In this gradation there is no room for another opposite. With the other senses of "possible" above there are good uses for "impossible", for example in the sense "logically impossible". We can say a statement is logically impossible when we know that it cannot be true because it is self-contradictory in form. Similarly there is a use for "impossible" as the complement of "possible" in the sense (described in the last paragraph) in which we can say that a certain statement cannot be true, not because it is self-contradictory in form, but because it purports to describe something that is incompatible with the character of the world as we know it.

It is often thought (and indeed is the founding principle of some systems of modal logic[2]) that "necessary," "possible" and "impossible" have a sort of complementarity, so that where we can use, say, "possible" we should also be able to use "necessary" and "impossible" and say something systematically different, of course, but of the same general type. That this is true only of some special cases I shall try to demonstrate. A very commonly employed grammatical construction in which all three words can be used is "It is . . . for . . . to . . . ". It will become clear on the examination of some actual examples that while we may fill the first blank of the above schema with "possible" and "impossible" and say the same *sort* of thing, filling the gap with "necessary" leads to statements of an entirely different kind.

Roughly, statements made with sentences using this construction with either "possible" or "impossible" filling the first gap express the compatibility or incompatibility respectively of what follows with the known character of the world. On the other hand, the use of "necessary" with this construction expresses some kind of obligatory condition. To see the watershed here, consider the kind of question appropriate after "It is possible for Nilotic tribesmen to jump twenty-foot railings" and the kind appropriate after "It is necessary for Nilotic tribesmen to jump twenty-foot railings". The former seems to admit of questions of the form "How do they manage such a feat?"—"*How* is it possible?"; while the latter seems to require questions of the form "Necessary for what?"—"*Why* is it necessary?" "Possible" and "impossible" exhaust the field of compatibilities with the known character of the world; since something is either compatible or incompatible with the way things are, there is no third possibility. The use of "necessary" here puts us into another dimension of language altogether. Clearly the complementarity of "necessary", "possible" and "impossible" cannot unhesitatingly be assumed.

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[2] J Lukasiewicz, *Proceedings of the XIth Congress of Philosophy,* Vol XIV, p 82

A clue to the discovery of what cases are in fact complementary can be obtained by returning to the performative notions which I suggested lay at the back of the use of modal expressions. The three notions from which I have derived the constellation of uses so far set out in this chapter were "obligation", "permission" and "prohibition". There is a very clear complementarity to be found with these concepts and indeed they can be transformed into one another by the use of appropriate negatives. However, it does not follow that because a simple complementarity can be found in these underlying notions that it has been preserved through the hypothetical etymology I have set out above. Some of the uses to which "possible", "necessary" and "impossible" come to be put may be such that the complementarity is lost.

The following equivalences seem to be acceptable:

(1) *X* is obligatory is equivalent to *not-X* is not permitted.

(2) *X* is forbidden is equivalent to *X* is not permitted.

(3) *X* is permitted is equivalent to *not-X* is not obligatory.
There are other possible equivalences too. However, this is a simplification of ordinary usage, as are most schemes of codification, for in this table *X*'s which are obligatory are included in *X*'s which are permitted. In ordinary speech we are inclined to use the words "permitted" and "obligatory" as if we thought the permitted and the obligatory formed exclusive classes. This conflation could be argued for on the ground that if an *X* is obligatory it is certainly not not-permitted. That this is true in some cases in ordinary speech can be seen from the existence of the apologetic "I didn't say you had to do it, I just said you could". (The ambiguity between "This ought to be done" as a command and as a recommendation).

There seems to be only one case where "necessary", "possible" and "impossible" form a system of uses such as that described above for "obligatory", "permitted" and "forbidden". This is in the case where they are used with "logically" in "logically necessary", "logically possible" and "logically impossible". This is an artificial system of uses, but it can be given a kind of translation into those uses of "necessary" and "possible" where they modify the ascription of "true" and "false", and where "impossible" is used in the sense of "cannot be true"; that is, in the case where "cannot be true" is used to mark an impediment that lies in the form of the sentence used and not in the world. We might say that "logically possible", "logically necessary" and "logically impossible" form a special set of truth-values.

When "necessary" and "possible" are used to mark the force or status of a condition (and this is a highly artificial use with "possible") "impossible" certainly cannot be used. It makes no sense to classify a condition as "impossible". When we do say the conditions are impossible we mean that *taken in themselves* they purport to describe states of affairs incompatible with the known character of the world. (Or in mountaineering or blackmail that the conditions are more than we can or are prepared to consider.) In fact "possible" does not go with "necessary" here either, for we tend to contrast a necessary condition with simply "a" condition or perhaps "an alternative" condition.

The breakdown of any smooth transition between the various alternative expressions for the Kantian modalities I cited at the beginning of this article can now be followed and explained in detail. We may dismiss Group I from this discussion, as they express modes of predication which we must take for granted, failing an analysis of atomic statements. Group II were those uses of modal expressions to modify the ascription of "true" and "false" which seem to be the only cases in which the complementarity of the underlying performative notions is preserved. I shall call this set the *logical* modes, for they are explicitly distinguished in technical discussions of logic (in contexts other than that of modal logic) by their being modified as "logically necessary", "logically possible" and "logically impossible".

In Group III "It is possible that *p*" and "It is necessary that *p*" are only superficially alike, for, in a real context where *p* is replaced by an actual sentence, "necessary" takes the subjunctive in the dependent clause and expresses the fact that the dependent clause is a condition for the attainment of some further end or aim. The similar construction with "possible" has no such sense but simply marks the compatibility of *p* with the nature of the world. This use of "possible" I shall call the expression of *empirical* possibility, in contrast to the use above, which is logical possibility. Similarly I shall contrast the necessity of an antecedent condition with the necessity to be true as the contrast between *anteconditional necessity* and *logical* necessity. (There is no sense to be given to empirical necessity except in the special way I shall discuss below.)

The adverbial uses of Group IV seem to express just what was expressed with auxiliaries in Group I. For example "Necessarily it is red" seems to express what is said by "It must be red" and "Possibly it is red" by one of "It may be red", "It might be red" or "It can (could) be red" It would be a mistake to suppose that there was a genuine complementarity in Groups I and IV. "It must be red" is used to give an order, to make a plea etc., while "It might be red" is used give expression to an envisaged possibility, or to express uncertainty about the colour of something. "It may be red" can be used to give permission for the colouring of something, and in this use *is* complementary to "It must be red". However, it is much more commonly used to make a guarded statement, similar to but not quite the same as the use of "might". Finally, "It can (could) be red" is commonly used as a direct statement of an alternative, expressing the speaker's belief that there are no obstacles in the way of the thing's being or becoming that colour.

The word "may" seems to deserve a closer analysis. This word can be used to do two different jobs. It has an independent use as a performative signal in the asking for and granting of permission. "May I go?" — "Yes, you may go". However, it is also used as an alternative to "will" in such statements as "It may rain" or "There may be war". It is used here, I think, as a way not of making a tentative prediction (the function of "might") but as a way of tentatively making a prediction. It is used to express hesitancy as opposed to probability or uncertainty (uses of "might").

The various ways in which modal concepts are used outside their original deontological setting can be summarized in a table. "O", "P" and "F" stand for "Obligatory", "Permitted" and "Forbidden" respectively, and I use them in the tables to indicate the *hypothetical* ancestry of the concepts there classified. An expression in brackets is an alternative that strikes me as less common.

Logical Modalities

	L I	L II
O	necessary (valid, analytic etc.)	must be true
P	contingent (possible, synthetic etc.)	can be true
F	self-contradictory (impossible etc.)	cannot be true

	L III	L IV
O	necessarily true	logically necessary
P	possibly true	logically possible
F	necessarily false	logically impossible

Empirical Modalities.

	E I	E II
O	—	—
P	possible	could (can) be true
F	impossible	couldn't (can't) be true

Epistemic Modalities.[8]

	EP I	EP II	EP III
	certain (definite)	is true	true
O	—	—	—
P	possible (uncertain)	might be true (may be)	possible
F	—	—	—

Anteconditional Modalities.

	A I	A II
O	necessary	necessary
P	is an alternative (sufficient)	unnecessary
F	—	—

Tentative Assertion Modalities.

	TA I
	will
O	—
P	may
F	—

In "true", "false" etc. we have a set of expressions the purpose of which is to express or mark the result of making an appraisal of the success or failure of statements and other locutions in fulfilling the task they are used for. The above expressions are used in constructions like "It is true that . . . " and "It is false that . . . ", which mark the result of making the appropriate appraisals of statements. It seems clear from the analysis so far that the modal notions of obligation, etc., are sometimes put to work in expressions like "It is true that . . . " and "It is false that . . . " to mark the results of a special class of statement-appraisals. It will now be obvious that this use does not cover all the ways in which modal notions enter our language, but it is an important way. In formalizing the use of modal expressions they must, therefore, in some cases be assimilated to the type of the truth-values of logic. From this preliminary conclusion about the logical status of modal expressions further conclusions can be drawn by a fuller discussion of the scheme set out above.

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[8] I borrow this term from G. H. Von Wright: *An Essay in Modal Logic*, pp. 1 and 2.

Logical Modalities. The sets of expressions in these use-families can be used for a variety of purposes.

1. They can be used to mark the outcome of the appraisal of a statement. This is clearly the case in such examples as "What he said must be true"; "That story can't be true".

2. It is usually the case that the appraisals marked by the use of these expressions are made, not on the basis of the "fit" of the statement and the facts, but on the basis of the internal structure and constitution of the statement; even, in some clear-cut cases, on the basis of the structure of the sentence used to make the statement. For example, a statement must be true if it is tautologous or analytic, and so tautology and analyticity in statements are *grounds* for the judgement expressed in "*p* must be true". Similarly a story can't be true if it contains some internal self-contradiction, if the teller of the tale contradicts himself, and so self-contradiction is a ground for the judgement "*s* can't be true". Clearly "must/can/can't be true" are not predicates which can be used to state the grounds upon which the appraisals they express are made; nor are the parallel forms "necessary", "contingent" and "self-contradictory" *when they are used as synonyms* for "must/can/can't be true".

The expressions of L IV, e.g. "*X* is logically necessary", are ambiguous unless carefully specified, for wo do not know *prima facie* whether they express a judgement arising from an appraisal of a statement or whether they are descriptive of the logical character and structure of *X* and hence serve as the grounds for such judgements. Similarly when we say "*p* must be true" we may either be appraising *p* or describing '*p*'. There is a still deeper complication inherent in these uses, for I have been talking here of statements, but the character of a statement with respect to necessity, contingency etc. is dependent in many cases upon the structure and grammatico-syntactical form of the sentence used to make the statement. Sometimes the expressions classified under logical modalities are used to describe the character of sentences, the sentences used to make the statements which "must/can/cannot be true".

Empirical Modalities. A similar ambiguity between judgement and grounds of judgement exists with the use-families E I and E II. While "can be true/can't be true" can only be used to express the judgements we make of statements (and metaphorically of sentences), "possible/impossible" can be used to make statements forming the grounds of these judgements. In this case the grounds lie not in the form of the statements made but in the compatibility of what is described in the statement with the known character of the world. I discussed above the case of certain tribesmen jumping railings. If it were said that they could jump seven-foot railings we might comment upon what is said with "Yes, it's just (quite) possible", for the situation imagined is compatible with the known character of the world, with what we know of athletic performances. However we would unhesitatingly say of the statement that they can jump *twenty*-foot railings that it can't be true. And the grounds for this judgement of the statement is the incompatibility of what is claimed with the facts of human athletic performance. It is worth noticing here that the impossibility of the alleged performance is not so strong as when a self-contradiction is the grounds for the adverse judgement, for we do not know *all* the facts about the world.

Epistemic Modalities. The pairs "certain/uncertain"; "true/possible" and "true/might be true" belong in the same dimension of language as "true" and "false", though they are used to express different dichotomous judgements. "True" in EP II and EP III seems to be used as shorthand for "known to be true", while "certain" and "uncertain" have a similar use. It is the expressions "known to be" and "might be" that are modal, for they qualify in certain ways the ascription of truth, that is they are adverbial in function. There is an analogy between "known to be" and "necessarily" that is not shared by "certain"; for with "know" we import the notion of obligation which is one of the basic modal notions. That which is known to be true is "obliged" to be correct; otherwise a serious impropriety has been committed by him who says he knows. "I know that . . . " is a performative formula that deserves an independent study.[4]

Anteconditional Modalities. These expressions are ways of bringing out the status of conditions. "Necessary", marking a condition without which the consequent is not to be achieved, is a natural extension of the basic deontological notions, for a necessary condition is an indispensable or "obligatory" one. A condition which is one among alternatives must, if it is to be a genuine alternative, be a sufficient condition. We do not usually use a modal adverb to mark this kind of condition, and it seems that this linguistic fact is connected with the meaning we commonly attach to "condition". A condition that is not a *genuine* alternative, say for a lack of sufficiency or perhaps irrelevance, we would hardly in ordinary language want to call a condition at all. The use of "unnecessary" in A II is connected with this point, for we use this word when we want to say that something or some action *serves no useful purpose.*

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[4] J. L. Austin, "Other Minds": *Logic and Language, Second Series* Ed. A. G. N. Flew, (Ch. 8) This contains an admirable account of the use of "know".

Tentative Assertion Modalities. The use of "may" to express a tentative assertion is a linguistic device somewhat similar in function to the use of "not" to express a negative assertion. That this is different from the use of "might" can be seen in the fact that "might" belongs in that spectrum of probability which begins with "certain" and tails off into uncertainty.

I suggested by the bracketing of "can" in E II that "can" is not so commonly used as "could". This is connected with the fact that we usually use "possible" and "impossible" in the envisaging of situations rather than in their description. There is thus very often a predictive element about the use of "possible" and "impossible", and this it is that tends to make us use "could" rather than "can" as the correct equivalent. "I can see now that it's possible", said as we watch the tribesman breasting the twenty-foot railings, is equivalent to "So it can be done", whereas an academic discussion of athletic possibilities must be expressed in terms of "could". There are fine shades of meaning here, for there are cases in which we express our wonderment when we had been *so* sure, as in the statement "So it could be done after all". This expression serves to refer back to our previous conviction that it couldn't be done, and I think simply reinforces the general analysis.

Natural Necessity. I suggested in the early part of this discussion that, though a good sense could be given to empirical possibility, natural or empirical necessity could only be given a sense in a very special case. This is important for modal logic, for if we interpret "possible" as an empirical modality the apparent complementarity between "possible" and "necessary" may lead us to interpret "necessary" in an empirical way. "Natural necessity" does not play any serious part now in the philosophy of science, but it does seem to enjoy a sort of half-life in uncritically interpreted modal logics because of the apparent complementarity above. (For example, it looks very much as if A. N. Prior is thinking of some sort of natural necessity in *Formal Logic,* p. 187, when he constructs a table of *general* equipollences between the various modal concepts.) There *is* a use of "must", and more rarely of "necessary", in scientific statements which deserves investigation. At an uncritical glance the use of these words might suggest that scientists were saying that certain phenomena *must* be the way they are; that they are "obliged" (by some natural necessity, as we might say) to occur the way they do. The word "must" occurs in this sort of example: "Given that the initial velocity of a bus is 10 feet per second and that it accelerates for 5 seconds at 2 feet per second it must have a velocity at the end of 5 seconds of 20 feet per second." (It is worth noticing that we do not commonly stress this "must" when *talking* like this.) I think it is proper to regard the necessity expressed in this use of "must" as coming from a rule or method of inference by which we infer the final velocity of the bus. If we accept a certain law of nature, in this case '$v=u+at$', as the proper rule for making predictive inferences about the kinematics of buses then *within the context laid down by this law* the conclusion follows necessarily. We are obliged to draw just this conclusion on pain of misusing the laws of nature and the rules of arithmetic. Clearly this does not further oblige us to accept "natural necessity", for that would mean accepting '$v=u+at$' and other laws of nature as themselves necessary. To this extent logic and physics are not parallel, for the general rules of inference which are the theorems of axiomatic logic are themselves logically necessary.

A distinction in kinds of modality was drawn by medieval logicians in terms of the modes of predication *de dicto* and *de re;* concerning a speech and concerning a thing. Clearly necessity can *only* be predicated *de dicto,* whereas we can say that possibility can be predicated both *de dicto* and *de re,* for "possible" can mean either "logically possible" or "empirically possible". This is still a gross over-simplification, as I have tried to bring out in the analyses above, but it serves to emphasize the point that only in the logical modalities is there a genuine complementarity between "necessary" and "possible". The attempts by modal logicians to express a *general* complementarity between modal notions using the language of "necessity" and "possibility" can only succeed at the price of an almost total distortion of the language they are supposed to represent.

Conclusions.

1. "Necessary", "possible" and "impossible" can only be used in the same scheme of interpretation for a logic when they mean: *either* "logically necessary", "logically possible" and "logically impossible", and here they belong in a ***meta-language*** used to characterize the statements of which they are predicated; *or* "must be true", "can be true" and "can't be true", and here they are of a type with the ordinary ascriptions of truth and falsity and must come into logic as special ***truth-values.***

2. There are many other uses of these words in our language, but in none do we find any kind of complementarity between "necessary" and "possible"; and it is on this complementarity that most systems of modal logic are based. It seems then that it is not possible to represent modal notions in a propositional calculus without giving that calculus an interpretation as a meta-language for a propositional calculus of a lower level.

3. A useful line of approach may lie in the concepts of "obligation", "permission" and "prohibition", but this raises new problems of analysis which are outside the scope of this article.

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DISCUSSION

DO WE EVER VALIDATE MORAL STATEMENTS?

I

By Robert Hoffman

Mr. D. H. Monro, in a critical notice of Dr. P. Edwards' *The Logic of Moral Discourse*, suggests a position which, though mistaken, is illuminating.[1] He maintains that if fundamental moral judgments[2] are logically implied by every moral judgment, and if we cannot validate them, we cannot be said ever to validate conclusions of moral arguments.

Now it is true, of course, that the validation of any non-fundamental moral judgment terminates with the exhibition of the principles that govern the realm of argument concerned. And, by definition, the principles which serve as fundamental moral judgments, i.e., the ultimate validating principles, cannot themselves be validated. Yet it does not seem to me to follow that we therefore cannot validate our non-fundamental judgments.

I think that Mr. Monro fails to take into account the practicality of the moral situation. What is called for is a decision here and now in terms of our doubts in particular circumstances: the doubts removed, we decide. And it must be emphasized that we do not begin with complete doubt; we bring to a situation certain funded values which we do not doubt. And, as Peirce points out, what we do not doubt, we regard as true. Moreover, doubt, when it is voiced, must be specific; it cannot be a general Cartesian doubt. This aspect of the matter has been put clearly by Dr. L. G. Miller:

"A request for justification [i.e., validation of a moral statement or act] is legitimate . . . only if the critic has grounds for doubt. When he questions the position it is implicit in his questioning that he has reasons for doubting and that it is in principle possible to remove this doubt by showing that it is unfounded. If this is not the case there would be no point in asking for justification. Furthermore, the critic cannot expect us to answer him until we know what reasons he has for doubting. Continued doubt is justified only if he has some grounds for doubt. If a person asks us to justify a moral statement it is always proper to ask him what his objection is and on what he bases it. If someone argues, 'No matter how many specific objections you answer, the validity of this judgment will still be in doubt' or 'Quite apart from specific objections, this judgment can still be doubted', he shows an unawareness of one of the basic characteristics of moral reasoning. Justification is associated with specific criticisms, whether actual or possible. If the critic is unable to raise specific objections against a judgment, or unable to give reasons why he thinks there may be objections as yet unrevealed, he cannot reasonably doubt the validity of the judgment. The judgment is not just probably valid, or highly valid — it is valid."[8]

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[1] This *Journal*, Vol. 34, No. 1, May, 1956.

[2] Dr Edwards defines a fundamental moral judgment as follows "When a man is unable or unwilling to support a moral judgment with anything that would be considered a reason we shall say he has made a fundamental moral judgment When and in so far as he can support a moral judgment with a reason, we shall refer to it as non-fundamental" (pp 182-183)

If, to the question "What have you in mind as an alternative position?", the critic of our judgment can offer no reply in terms of a judgment and reasons therefor, he is not (logically) entitled to entertain doubt. The presumption of truth for our judgment stands until he overrules it by a countervailing claim, i.e., an opposed judgment supported by a reason or reasons.

But suppose countervailing claims are urged until the moral dispute in question is fundamental. Moral conflicts of this sort *are* incapable of being settled in any rational way. Doesn't this show that moral disputes are, after all, irrational, for any dispute can be imagined to be pursued until it becomes a fundamental one? I think not.

First of all, validation must come to an end somewhere or else it would not be validation at all. Consider the problem with which the Tortoise confronts Achilles in Lewis Caroll's "What the Tortoise said to Achilles". He offers Achilles three propositions, A, B, and Z:

(A) Things that are equal to the same are equal to each other.

(B) The two sides of this Triangle are things that are equal to the same.

(Z) The two sides of this Triangle are equal to each other.

Then he tells Achilles that he wishes him to suppose that he (the Tortoise) accepts propositions A and B as true but does not accept the hypothetical proposition that, if A and B are true,

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[8] Miller, L. G, *Some Problems Concerning the Justification of Moral Rules.* Unpublished Ph.D. Thesis, Cornell University, 1954, p. 155; cf. Toulmin, S., 'Knowledge of Right and Wrong', *Proceedings of the Aristotelian Society,* Vol. L (1949-1950), p 155

Z must be true, and then force him logically to accept Z as true. The following dialogue ensues:

A: I'm to force you to accept Z, am I? And your present position is that you accept A and B, but you DON'T accept the Hypothetical . . .

T: Let's call it C.

A: . . . but you don't accept: (C) If A and B are true, Z must be true.

T: That is my present position.

A: Then I must ask you to accept C.

T: I'll do so as soon as you've entered it in that notebook of yours. Now write as I dictate: (A) Things that are equal to the same are equal to each other. (B) The two sides of this Triangle are things that are equal to the same. (C) If A and B are true, Z must be true. (Z) The two sides of this Triangle are equal to each other.

A: You should call it D, not Z. It comes NEXT to the other three. If you accept A and B and C, you MUST accept Z.

T: And why MUST I?

A: Because it follows LOGICALLY from them. If A and B and C are true, Z must be true. You don't dispute THAT, I imagine?

T: If A and B and C are true, Z MUST be true. That's ANOTHER Hypothetical, isn't it? And, if I failed to see its truth, I might accept A and B and C, and STILL not accept Z, mightn't I?

A: You might, though such obtuseness would certainly be phenomenal. Still the event is POSSIBLE. So I must ask you to grant one more Hypothetical.

T: Very good. I'm quite willing to grant it, as soon as you've written it down. We will call it: (D) If A and B and C are true Z must be true.

A: Now that you accept A and B and C and D, OF COURSE you accept Z.

T: Do I?[4]

Now, as Achilles soon finds out, D is followed by E and E by F etc., *ad infinitum*. The moral of the tale: beyond a certain point, namely, the paradigm case, it makes no sense to ask for proof. You can't ask why the hypothetical in question is sound:

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[4] *Mind*, 1895; *Complete Works of Lewis Carroll*, Modern Library, pp 1226, 1227-9

the hypothetical is the "limit" of the proof; beyond it, there is nothing in terms of which it can be validated.

Similarly, the fundamental moral judgment cannot be validated. This does not mean that everything that follows from it is therefore unvalidated. It means only that in reaching the fundamental moral judgment we reach the terminating point of our validation. We cannot have it both ways: either there is an ultimate validating principle, i.e., a terminating moral judgment, or there is no validation. The cry for no ultimate judgment, i.e., for a reason for *every* moral judgment, *and* a validating procedure is irrational. It is like asking for the last member of an infinite series. We must reply: "There is no such animal; there can't be".

Moreover, moral theory is not alone in its reliance on the unproven. The difficulty would arise in natural science if we were to ask for a justification of induction (i.e., of a rule such as the following, which underlies scientific method: "We may argue from 'most instances of X's examined in a wide variety of conditions have been Y' to (probably) 'the next X to be encountered will be Y' ") before we made scientific predictions, on the ground that if the rule is unwarranted it makes no sense to base predictions on it. And in pure mathematics, Gödel's demonstration that there exist undecidable formulæ, i.e., formulæ constructive within a given system S, such that neither the formulæ nor their negations are demonstrable within S, might lead someone who insisted on determining beyond any possible doubt whether S is consistent to a difficulty of a similar sort. For Gödel demonstrates that another formal system, i.e., a meta-system S_1 could be employed to demonstrate the consistency of S. But suppose someone asks whether S_1 is consistent. If it is not, then any arbitrary formula could be deduced from its axioms, including the formula which says that S is consistent. So we should have to show that S_1 is consistent by using still another system, S^2, etc.[5] Paging Achilles and the Tortoise!

Thus, the demand in any field for a reason for an *ultimate* judgment, rule, or axiom is senseless. If there were a reason, the judgment, etc. would not be ultimate. It is not, then, a matter — to get back to ethics — of the fundamental moral judgment being "irrational" in any pejorative or commendatory sense of that term. We cannot call it "irrational" in any sense other than a definitional one. And when we see that this is so, we realize that we err logically if we apply the term "irrational" in a valuational sense to a *fundamental* moral judgment. And it follows at once that we cannot call an entire ethical system "irrational" in a valuational sense because its fundamental moral judgment is not supported by reasons. Nor can we, of course, call the *system* "irrational" in the descriptive sense, because if logically proper as opposed to merely causally influential reasons are offered in support of non-fundamental judgments in the manner suggested by Dr. Edwards, the system is rational, no matter what the epistemological status of the fundamental moral judgment.

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[5] Waismann, F, *Introduction to Mathematical Thinking*, New York, Ungar, 1951, pp 100ff

Elmont, New York.

II

By D. H. MONRO

Mr. Hoffman takes me to be making a general point about all reasoning; I was actually making a specific point about moral reasoning.

It is no doubt true that statements are validated by showing that they follow from other statements taken to be true. These may be validated in their turn; but obviously there must be at least one premise which cannot be validated in this way, but which must, it seems, just be taken for granted. Something else is taken for granted here too: the validity of the rules of inference which enable us to say that one statement "follows from" another. Mr. Hoffman does not distinguish between these two; but it is as well to notice that his example from Lewis Carroll, and the one about induction, apply to inference rules, whereas his main argument is about the validation of basic premises.

So far I have been talking about reasoning in general. But there is, I think, an important difference between moral reasoning and reasoning about matters of fact. When our reasoning is about a matter of fact, we shall, if we go on validating our premises long enough, eventually arrive at a basic premise that rests on the evidence of the senses: an observation-statement. With moral reasoning, on the other hand, the basic premise must be a statement which itself contains a moral term: a value judgment. And, whatever value judgments are, it seems clear that they are not observation-statements.

The problem of validation, then, may arise about any of these three: (1) rules of inference, inductive or deductive; (2) observation-statements; (3) value judgments. Mr. Hoffman assimilates all three, and takes me to be voicing a general, philosophic doubt which would apply indifferently to any of them. Now it is, of course, quite possible to raise philosophic doubts about both the rules of inference and the evidence of the senses. But these are not the doubts I was raising. I am quite happy (as a man if not always as a philosopher) to take both the evidence of my senses and the accepted rules of inference for granted. But I do feel uneasy about taking value judgments for granted. And I feel uneasy just because I am concerned with what Mr. Hoffman accuses me of ignoring: "the practicality of the moral situation".

It is quite true, as Mr. Hoffman says, that "we bring to a situation certain funded values which we do not doubt". But what if we are confronted with other people who do doubt them, and who have a set of values which *we* doubt? As I said specifically in my review of Edwards, this does not usually happen with induction. But it does happen with morals. Quite obviously, it happens to Dr. Edwards. Throughout his book he is anxious to defend a particular set of values against people in his own community who oppose them. He thinks that his values are rational and that theirs are not. As it happens, I am on Dr. Edwards' side in the practical controversy. But I do not see how he can combine this position with a meta-ethic which leads to the conclusion that, in Mr. Hoffman's words, "we err logically if we apply the term 'irrational' in a valuational sense to a fundamental moral judgment". Mr. Hoffman draws the comforting conclusion that we need not worry about our own value judgments being irrational. But of course it also follows that we cannot accuse our opponents of being irrational either. Perhaps it is just intolerant of me to be unhappy about this, but at any rate I do not see how Dr Edwards can have it both ways.

This is of course an *argumentum ad hominem* against Edwards (though it happens to be the point I was making in the passage that Mr. Hoffman criticises); and it may not apply to Mr. Hoffman. But I find Mr. Hoffman's own position rather puzzling. In the main, he seems to think that any doubts about fundamental moral judgments are on a par with the doubts that Lewis Carroll might raise, as a kind of logical joke, about deduction; or perhaps with those that Descartes might raise, as a kind of exercise in doubting, about the senses. The assumption is that our "funded values" are at least as well grounded as observation-statements or the rules of inference, that there is no practical (as distinct from philosophical) reason for questioning them. It is odd to take this line in defence of Edwards; because it is (or is at least very like) the position of the intuitionists, whom Edwards attacks so bitterly. Intuitionists, too, are fond of comparing fundamental moral judgments with the rules of inference; they say, like Mr. Hoffman, that neither can be proved, but that neither need to be; they add (what he perhaps implies) that both are self-evident. And here one difficulty is that the difference (e.g.) between Edwards and his opponents (the puritans: in his more colourful language "the fuddy-duddies and the sourpusses") may well turn out to be a difference in their fundamental moral judgments. What is self-evident to him is very far from being so to them.

There is, however, at least a hint of a different position in Mr. Hoffman's article: that people do sometimes make different fundamental moral judgments, and that, when this happens, one judgment is as good as (or at least as "rational in a valuational sense") as the other. This is of course the subjectivist position, and it seems to be Dr. Edwards'. There are certainly strong arguments for it; my chief complaint against Edwards was that he did not seem to recognise its full implications. But I do not think that this position can be defended by an analogy with rules of inference or observation-statements. Would even the most inventive framer of deductive systems say seriously that any rule of inference was as rational as any other? Or that it is as rational to trust the authority of a sacred book as the evidence of the senses? We do not accept the evidence of the senses or the rules of inference just because we must start *somewhere,* and must have *some* rules of procedure.[1] We accept them because they work, and it is much easier to see what this means than to see what it means to say that a set of moral values works. This is, however, a large subject which I had better not try to pursue here. I hope I have said enough to suggest to Mr. Hoffman that the validation of fundamental moral judgments presents different problems from the validation of either the rules of inference or the basic premises of empirical arguments.

Sydney University.

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[1] See (by a pleasing coincidence) Mr Goddard's article in this issue

A NOTE ON QUESTIONS

By C. F. Presley

I wish to comment on two of the postulates put forward by Dr. Hamblin in his admirable discussion of the logic of questions.[1]

"*Postulate* 2. Knowing what counts as an answer is equivalent to knowing the question" (p. 162).

"Knowing what counts as an answer" might be taken to mean (1) "Being able to state all the possible answers to the question", or (2) "Knowing what form the answer will take". "Form" is used here in the sense in which "Luxembourg is in the continent of . . . " is the form of the possible answers to the question "In which continent is Luxembourg?" It should be made clear that if Postulate 2 is to be satisfactory, it must sometimes be interpreted in the one way, and sometimes in the other. This is because on interpretation (1) it is, in some cases, possible to know a question without knowing what counts as an answer, while on interpretation (2) it is possible to know what counts as an answer without knowing what the question is.

I may understand the question "In what state is Detroit?" (a), although I cannot list all the possible answers to this question because I do not know the names of all the United States. I also understand the question "What is the mean diameter of the earth at the equator?", although in this case I cannot state all the possible answers because there is no end to them. In both of these cases interpretation (2) is satisfactory: I know that the answer to the first question will be of the form "Detroit is in the state of . . . ", and that the answer to the second will be of the form "The mean diameter of the earth at the equator is . . . metres".

But there are cases where interpretation (2) will not do. Knowing that the answer to a question is of the form "John is . . . swimming", I do not know whether the question is "Is John still swimming?", or "Is John swimming already?" Here interpretation (1) is necessary: in order to know that the question is "Is John still swimming?" I need to know that the possible answers are "John is still swimming" and "John is no longer swimming".

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[1] Hamblin, C. L., "Questions", this *Journal, Vol* 36, *No* 3, (*December,* 1958), pp. 159-168

The Detroit example (a) might suggest that in some cases one can know the possible answers without knowing the question. I am told that the possible answers to a question are "Detroit is in Ohio", "Detroit is in Vermont", "Detroit is in Texas", and so on, but I may not know that the question is (a) just because I do not know that the second name in each possible answer is the name of one of the United States. Nevertheless, from any given set of possible answers it is possible to construct a question by deriving a yes-or-no question from each possible answer and *joining* (see p. 166) all the questions so obtained: e.g., (b) "Is Detroit in Ohio, or is it in Vermont, or is it in Texas, . . . ?" If one accepts that (a) and (b) are different formulations of the same question — I take this to be Hamblin's view — then on interpretation (1) it is true that if one knows what counts as an answer then one knows the question. But it is only on interpretation (2) that knowing what counts as an answer follows from knowing the question. Philosophical questions give rise to special difficulties here.

"*Postulate* 3. The possible answers to a question are an exhaustive set of mutually exclusive possibilities "(p. 163). From this it follows that "If a question has only one possible answer, that answer is a tautology" (p. 164).

Consider the question "Can you hear me?" (c). In certain circumstances the only possible answer to (c) would be the contingent statement "I can hear you". But in unusual circumstances — where the person addressed is able and in a position to lip-read, for example — there will be two possible answers, and then, of course, the above theorem will not apply to (c). A question to which the theorem applies unconditionally, and of which it is false, is the less likely "Are you aware that I am trying to communicate with you?" (d). If the person addressed is aware of this attempt to communicate with him, then the answer is "Yes", but if he is not, then, even though by chance he were to say "I am not aware that you are trying to communicate with me", this statement would not be an answer because it would not have been made *in answer to* a question. So the only possible answer to (d) is "Yes", or more fully "I am aware that you are trying to communicate with me", which is not a tautology.

Associated with a question is an exhaustive set of mutually exclusive statements: let us call these "A-statements". According to Postulate 3, the A-statements associated with a question are the possible answers to that question. It is only if certain conditions are satisfied that a statement can function as an answer: let us call the statement of these conditions "C". If C is incompatible with one of the A-statements, then that A-statement is not a possible answer to the question; i.e., Postulate 3 breaks down. In such cases the disjunction of possible answers is not a tautology, and consequently even if such a question has only one possible answer, that answer will not be a tautology.

If (c) and (d) and others very like them are the only questions of this kind, it would be advisable to retain Postulate 3 and to note that (c) and (d) are not questions of the kind implicitly defined in Hamblin's system. This step is reasonable because (c) and (d) are more like call-signs than questions: they are ancillary to, rather than a part of communication, serving to make contact rather than to elicit information.

I have not discussed "Is this a question?", which is like (c) and (d) in some ways, or "Is your answer to this question true?", which is different, because they both differ from (c) and (d) in that we have no use for them.

Adelaide University.

MR. ARTHADEVA AND NAIVE REALISM

By D. M. ARMSTRONG

Mr. Arthadeva[1] is concerned with the difficulties that mirror-images raise for Naive Realism. Many philosophers have argued in the following way:

1. Objects "in the mirror" are *seen* just as much as anything else in our field of view.

2. Objects "in the mirror" are not physical objects. On this basis they have concluded that:

3. The true objects of sight are not physical objects. They then wander in the wilderness prepared for them by Locke and Berkeley.

Mr. Arthadeva agrees with the first step in this argument, but at the same time he wants to deny its conclusion. As a Naive Realist he wants to say that what we perceive is always something physical. He therefore argues:

1. Objects "in the mirror" are *seen* just as much as anything else in our field of view.

2. The true objects of sight are physical objects. On this basis he concludes that:

3. Objects "in the mirror" are physical objects.

Now this undoubtedly is a very tempting line of argument. Mirror-images do seem to be at least quasi-substantial. Many philosophers are therefore prepared to treat them as junior physical objects (Mr. Arthadeva points to the example of Professor J. L. Austin). Nevertheless, this line of argument has to face difficulties. Suppose I am two feet in front of a mirror. My mirror-image appears to be *two feet behind the surface of the glass.* Now the actual physical objects that occupy the space two feet behind the surface of the glass may well be such as to render it quite impossible that my mirror-double should be there too. (Suppose, e.g., the mirror is hanging on a wall which is more than two feet wide.)

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[1] This *Journal*, December, 1957

Mr. Arthadeva offers an ingenious attempt to escape this difficulty. He suggests that when I look into a morror I am simply seeing ***myself.*** I am simply getting a view of ***myself*** that I am not normally able to get, *viz.* the view another person has when he faces me. And if I see myself directly and by means of the mirror at the same time, then I am simply in the fortunate position of getting two different views of the same object. Mr. Arthadeva is able to support his case by an interesting piece of linguistic evidence. We say "I saw myself in the mirror" and not "I saw an illusion", or "I saw another person who resembled me".

But there remains an important difficulty. Do not I and my mirror-image appear to be at *two different places?* And how can the same thing be at two different places at once? Mr. Arthadeva tries to get over this difficulty by arguing that the mirror-image does ***not*** seem to be in a different place from where I am. Place, he points out, is relative, and is determined by a thing's relations to other things:

> "Where, for instance, is my shoe? It is here (in front of the mirror): that is, between the glass before me and the wall behind, and between the chair on one side of me and the table on the other. It is *there* ('within the mirror'): that is, between the glass and the wall and between the chair and the table."

The description of "here" (in front of the mirror) and "there" ("within the mirror") are identical, therefore "there" is identical with "here".

CRITICISM OF THIS SOLUTION

Now I think we can say that when we look into a mirror what we see is ourselves. But if we do say so, we shall have to realize that we see ourselves *subject to certain distortions,* a distortion of place and the characteristic left-right distortion.

It is undoubtedly true that if we consider only the relations of an object A in front of the mirror to other objects also in front of the mirror, on the one hand, and the relations of the mirror-image of A to the mirror-images of the other objects, on the other hand, then there is no discernible difference between A and its mirror-image. But it is possible to show they are two in another way. If I stand two feet in front of the glass, then, as has already been pointed out, my mirror-image seems to be two feet behind the glass, and inverted, ***which means that it seems to be four feet away from me, and inverted relative*** *to me.* Now a thing that is four feet from me, to take only the first point, cannot be identical with me. Hence it must be admitted that when I look into a mirror there is at least an *element* of illusion. (This point tends to be obscured by the fact that we are very familiar with mirror-images, because of their usefulness in the conduct of life. But a child or dog, on first seeing a mirror, will think there is *another* child or dog *in front of itself.* This can happen to us, too, if we do not know there is a *mirror* in front of us.) Hence Mr. Arthadeva's solution fails.

AN ALTERNATIVE ACCOUNT.

I suggest that both the opponents of Naïve Realism, whom Mr. Arthadeva attacks, and Mr. Arthadeva himself, make the same mistake when they accept the premiss:

1. Objects "in the mirror" are *seen* just as much as anything else in our field of view.

For when objects seen by looking into a mirror are "seen" as "in the mirror" they seem to be behind the glass of the mirror. But there are no such physical objects behind the glass. Now I agree with Mr. Arthadeva that the only things we can see are physical things, hence I draw the conclusion that in such cases we do not see, but only *think* we see, an object four feet in front of us. And if it be replied that it seems to us to be seeing, the answer is that of course it *seems* so to us, for if it did not we would not think we were seeing. The cause of this false belief that we are seeing something is a matter for psychological investigation, and I have discussed the question elsewhere.[1]

This solution of the problem about mirror-images has to face one apparently very damaging objection. As already remarked, mirror-images deceive only the uninitiated, and the average person who turns his eyes towards a mirror does not believe that there is an object like himself behind the glass. How, then, can he think he is seeing such an object?

But when we look at mirrors it is *as if* we were looking at an object like ourselves but in front of ourselves, and it is only because we can bring all sorts of independent knowledge of the world to bear on the situation that we are not deceived. But for such independent knowledge it is logically necessary that we should be deceived. This suggests that we can say that when we turn our eyes towards a mirror we have a

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[1] This *Journal,* August, 1955, pp. 101-3.

temptation or *inclination* to believe falsely that we are seeing an object like ourselves but in front of ourselves.

In this way we can do justice to the element of illusion normally involved in looking at mirrors (an element which Mr. Arthadeva wrongly denies to be present), while at the same time avoiding the postulation of non-physical objects of vision. To speak harshly, there are no such things as mirror-images, there are only people who believe falsely or are inclined to believe falsely that they are seeing certain things that do not exist behind the surface of pieces of glass. The word 'mirror-image' has a use, but no substance corresponds to the substantive.

University of Melbourne.

CRITICAL NOTICE

THE MORAL POINT OF VIEW: A RATIONAL BASIS OF ETHICS. By Kurt Baier. Ithaca, Cornell University Press, 1958. xii, 326 p. $4.

Professor Baier writes clearly, unpretentiously and incisively; and he combines common sense with considerable analytic subtlety. His handling of the distinctions between related terms (such as "regulations", "mores", "principles" and "canons") is always acute and usually illuminating. He is admirable at breaking down a complex question (such as: "Is morality prior to society?") and showing the ambiguities that lurk within it. He can also put the essentials of a problem very neatly and clearly: his preface, which enumerates the "four main logical features of moral judgments" and points out that none of the traditional moral theories can account for them all, is a remarkably concise and penetrating statement of some of the central problems of moral philosophy.

Sometimes, it is true, one doubts whether his careful dissection of terms really does enable him to make the point he is trying to make; one may even suspect that the main thread of the argument has rather got lost during the patient examination of side issues. But the other side of this is that, even if one is dissatisfied with Baier's main argument, one can still find much of value in these incidental discussions. Since it is the main argument that I want to discuss, it is only fair to say this at the beginning.

What Baier is really trying to do, it seems to me, is to reconcile two very different, and at first sight incompatible, moral theories. One of these derives from Toulmin and, more remotely, from Bishop Butler: it might also be called "Ross without intuitions". The other is a modification of Hobbes. Since each of these theories stresses features of morality which the other ignores, the attempt to reconcile them is of great interest. I should perhaps make it clear that Baier does not himself explicitly advance these theories: in talking about them I am merely making explicit what seem to me — possibly not to him — the two main strands of thought in this book. The terms in which I state them are my own, which may not always be his; and, for ease of exposition, I am putting each theory in an extreme form, which he might want to modify.

Theory A: The difference between value judgments and empirical assertions has been much exaggerated. We test empirical assertions by applying the appropriate criteria: saying, for example, "This building is taller than that; it casts a longer shadow." We test moral assertions in the same way: saying, for example, "You ought to return the book; you promised to." Moral criteria can be questioned; but so can empirical criteria. The validity of empirical criteria depends in part on the meaning of words like "tall" and in part on empirical assertions, which can be tested in their turn, about such things as shadows. The validity of moral criteria depends on the meaning of moral terms and on empirical assertions about such things as human aspirations and emotions. The rule about keeping promises, for example, can be tested both by its consequences and by its fitting into a whole system of similar rules: the system as a whole constitutes "the moral point of view". The moral point of view can best be understood by describing it. Briefly, it is in Baier's words, the point of view of "an independent, impartial, objective, dispassionate, disinterested observer" (p. 201). Now, if anyone asks "But ought we to adopt that point of view?" the answer is that this is a meaningless question. "Ought" is itself a moral term, and to ask whether we ought to do something is to ask whether doing it accords with the moral point of view. If anyone asks: "Is a six-footer *really* taller than a man who stands four feet six?", the answer is: "Well, that's just what we mean by 'taller' ". And, if anyone asks: "Is the moral point of view *really* better than any other?", the answer is "That's what we mean by 'better' ". Or, as Baier puts it: "How could there be such a justification? It cannot be a justification in terms of self-interest, for it must be admitted that morality does not necessarily pay. Nor can it be in terms of moral reasons, for we are now talking *about,* not *within* morality. 'Why should I be moral?' cannot mean 'Does being moral pay?' or 'Is there a moral reason for being moral?' But it cannot mean anything else either, for there are no other types of reason. Hence it is an illegitimate question." (pp. 13-14.)

Theory B: Men live in societies, and their lives would be very miserable if they did not. If a society is to keep going, its members need to accept and, in the main, observe a set of rules about their behaviour to one another. It is these rules that we call 'morality'. Different societies may, up to a point, have different moralities; but there are some fundamental rules which should form part of the morality of any society. Otherwise the lives of its members will not be tolerable, and the society in question will be an immoral one. To ask whether an action is right is to ask whether it fits in with such a system of rules. The reason why the rules should be observed is that it is one's interest to live in a tolerable society.

Baier's position, as I understand it, is that theory A will do very well as an account of morality, from the point of view of morality itself. But the moral point of view does, he thinks, need the kind of justification that theory B can give it. We understand the moral point of view once we realise that it is forced on us by the needs of society; and we satisfy the needs of society because it pays us to. But he does not draw the conclusion that morality is ultimately justified by self-interest. What justifies both morality and self-interest is reason; and reason really is self-justifying, though morality is not.

Now, theory B does correct some of the defects of theory A. Philosophers nowadays often write as if moral principles were things, like trees or animals, which we just find lying about. They examine them carefully and report on their distinctive features (universalisability or whatever) much as biologists might report on the cellular structure of trees or animals. The biologist may, indeed, ask the further question: How does this animal come to have this structure? But there can be, for him, no question of *justifying* the structure. If anyone is misguided enough to ask: "But *ought* this animal to be like this?", he will shrug his shoulders and say: "Well, that is what it *is* like, and that is that". Similarly, many philosophers are inclined to say: "Moral rules just are universalisable, and the question 'Ought they to be?' just doesn't make sense."

Moral principles, however, are not like trees or animals. We don't just stumble across them. We make them. If we don't, as individuals, at least men collectively do. Following moral principles is a human activity. And we can't take human activities for granted, because presumably it is in our power to alter them, or to abstain from them, if we choose. If it is possible to order our lives by principles with a different set of formal features, someone sooner or later is going to ask the question: Shall we? And this is not the linguistic question: If we do, will the descriptive label "moral" still apply to our new principles? To say that we ought to follow a principle is not just to say that it has certain descriptive features (is "impartial", "disinterested", etc.). It is to claim that it should take precedence over other kinds of principle, when there is a conflict. And this claim does, on the face of it, need justification.

It is here that Hobbism (theory B) comes in, providing us with both an explanation and a justification of morality. If we ask: "Why are we moral?" then the answer is, as Baier notes, that "we have been trained to regard moral reasons as superior to all others, and we have accepted that" (p. 296). Men have trained their children in this way because they have come to realise that moral rules are essential to society, even the restricted society of the family. We can also understand most of the characteristic features of moral rules (universalisability, impartiality, and so on) once we realise that, to fulfil their social function, moral rules need to be teachable, publicly promulgated, and acceptable to everyone. And, if we ask why we *ought to be* moral, the answer is that morality is essential to society, and that we would be very unhappy without society. The ultimate justification, then, is self-interest.

But this is the snag. For, while it is no doubt to one's interest that other people should be moral, and worth being moral oneself to ensure that they are, occasions will arise when one can break a moral rule without immediate harm to oneself, and with no serious danger to the stability of society. Self-interest, then, even when enlightened, will sometimes conflict with morality. Or, as Baier puts it: "The best possible life *for everyone* is possible only by everyone's following the rules of morality, that is, rules which quite frequently may require individuals to make genuine sacrifices" (p. 315). It follows that "the moral point of view", the point of view which sets the best possible life for everyone above the best possible life for oneself, is not always justified by self-interest. Self-interest, then, cannot be the ultimate justification of morality.

It may be doubted whether this argument is as conclusive as it is generally thought to be. For, the consistent Hobbist might ask, why should one not exempt oneself from the moral rules on the rare occasions when no harm to oneself will result, either directly or indirectly? One can retort, of course, that it is not *right* to do so, or that doing so would outrage one's moral sense, but these assertions, though quite true, are beside the point. For "right", in this sense, means "in accordance with moral rules": in accordance, that is to say, with the rules which society promulgates for its own protection. And, of course, it could not be one of those rules that they need not be observed. As for the moral sense, that is the psychological result of one's childhood indoctrination in the moral rules themselves; so of course it is outraged by any proposal to break those rules. The only sense of "right" (or "ought") which is relevant here is quite a different one, in which it does make sense to ask whether it is right to break the moral rules. In this sense "right" means something like "rational". Now, if we ask whether it is rational to make exceptions in one's own favour (always assuming that one can do so without harmful consequences to oneself) the answer, on Hobbist principles, must be "yes". For, if it is rational to accept the rules of morality on the grounds that accepting them is the only condition on which there can be a society, and that society is essential to one's well-being, then it is rational to break those rules if one can break them without endangering the stability of society. The rational man will not, of course, advise others to break them even in these circumstances: for that would be against his own interests. But if he can break the rules in secret he will do so, while at the same time advising others always to keep the rules, in all circumstances.

Baier, however, is not prepared to follow Hobbism so far. He is quite clear that, when morality conflicts with self-interest, one ought to prefer morality. Yet he is not quite happy about the contention that it does not make sense to ask for a justification of morality itself. For moral reasons, he points out, are only one kind of reason for action. There are also reasons of self-interest: for example, that one enjoys doing something is a reason for doing it. ("Reason" here, it should be noted, means something like "good reason" or "justification": Baier distinguishes sharply between the causes of our actions and the reasons for them. The one word, he says, is explanatory: the other justificatory.) That another person enjoys your action is also a reason for doing it. But the first of these, Baier tells us, is a better reason than the second: there is no reason why you should sacrifice your own pleasure for the sake of an equal amount of pleasure for a complete stranger who has no claims on you. Finally, there are moral reasons; and these are better than either of the other two.

Now, "better" here can hardly mean "morally better", since the comparison is between moral reasons and other kinds of reason. Nor can it mean better from the point of view of self-interest, or of altruism. What then does it mean? Like the imaginary Hobbist I invoked above, Baier says that it means "more rational". It is "a rule of reason" that my enjoyment of an action is a reason for doing it. Moral reasons are also rules of reason. And it is a further rule of reason that moral reasons are superior to all others. The supremacy of morality over self-interest, then, has the same justification as has self-interest itself.

Morality, then, can be validated after all: by an appeal to reason. But how is reason to be validated? Now here, Baier says, we really do have an illegitimate demand. "Ought I to be moral?" is not a tautological question, since "ought" means, not "in accordance with morality", as has been thought, but "in accordance with reason". But it follows from this that "ought I to follow reason?" really is a tautological question. It is "as silly as 'Why is a circle a circle?' " (p. 317).

Now this is certainly more plausible than the parallel contention about morality, which it replaces. But can we be sure that the rules of morality are guaranteed by reason? Baier's argument here would certainly not satisfy the Hobbist. For what he does is to argue the case for saying that it is rational (other things being equal) to do what you enjoy doing. Then he says: "I need not waste any further time on examining whether the other consideration-making beliefs prevalent in our society are true. Everyone can conduct this investigation for himself" (p. 304). This is a very shaky foundation for the moral reasons and an even shakier one for the crucial contention that moral reasons are superior to reasons of self-interest.

What Baier actually appeals to when he invokes reason seems very little different from Ross's "intuition". Baier disputes this, but his argument turns on giving to "intuition" a meaning that Ross would never give it. He takes as the paradigm case of "intuition" the achievement of the mathematical prodigy who gives an instantaneous and correct answer to complicated arithmetical questions. But for Ross the paradigm case would be our grasp of the rules of inference. The propositions that, according to him, we "know by intuition" are those that are usually called "self-evident". Now, "that I enjoy something is a reason for doing it" would really do very well as a paradigm case of a self-evident proposition. Moreover, it might be contended that the refusal to accept a self-evident proposition is a paradigm case of "being irrational", "refusing to follow reason". I doubt if Baier's argument amounts to more than this. Anyone who seriously rejected the proposition "that I enjoy something is a reason for doing it" would, he says, be mad. But, if this is the test of our "consideration-making beliefs", will moral reasons pass it? People who reject moral reasons are not mad; they are immoral.

Baier has an additional argument for saying that it is a rule of reason that moral reasons are superior to reasons of self-interest. In considering this question, he says, "we are now looking at the world from the point of view of *anyone*. We are not examining particular alternative courses of action before this or that person; we are examining two alternative worlds, one in which moral reasons are always treated by everyone as superior to reasons of self-interest and one in which the reverse is the practice. And we can see that the first world is the better world, because we can see that the second world would be the sort which Hobbes describes as the state of nature" (p. 310).

But why is this the question we have to examine? Baier can say, of course, with considerable force, that the *moral* question is the one that he poses; for to take the point of view of morality just is to take the disinterested, impartial point of view: to prefer "the best possible life for everyone" to the best possible life for oneself. But the question here is whether that point of view is also the rational point of view. And Hobbes is no help here; for Hobbes' point is simply that, because he needs the co-operation of other men, the individual cannot hope to secure the best life for himself if it conflicts with the best life for everyone. Baier, however, disagrees with this. "A person", he says, "might do better for himself by following enlightened self-interest rather than morality. It is not possible, however, that *everyone* should do better for himself by following enlightened self-interest rather than morality. The best possible life *for everyone* is possible only by everyone's following the rules of morality, that is rules which quite frequently may require individuals to make genuine sacrifices" (pp. 314-5). It seems clear from this that, in Baier's view, it is rational to aim at the best possible life for everyone, not merely as a means to the best possible life for oneself, but even at the cost of forgoing the best life for oneself. Hobbes' argument does not establish this, nor would Hobbes himself accept it. And it is not enough simply to say, as Ross might, that we are all convinced that this assertion is true, that the contradictory belief outrages our moral consciousness. For, as I have mentioned above, the Hobbist can account for this: all that it means is that, in Baier's words, "we have been trained to regard moral reasons as superior to all others, and we have accepted that". But not every belief that we have been trained to accept is rational.

It is even possible that Baier is confusing two quite different things, either of which might be meant by "the point of view of anyone". In the passage quoted above from p. 310, in which that phrase is used, he is talking about principles which, if they apply to anyone, apply to everyone. In the passage from pp. 314-5, he is talking about the *interests* of everyone. Now, it is true that, if it is rational for me to put my own interests before the interests of everyone else, it must be rational for others to do the same. In that sense, the rational point of view is "the point of view of anyone". But it does not follow that it is "the point of view of anyone" in the other sense, the point of view of the general interest, or the best possible life for everyone, as distinct from the individual interest, or the best possible life for oneself. What does follow is that it is rational, not just for me, but for *anyone who can,* to secure his own interests at the expense of everyone else. Hobbes adds to this that in fact no-one can, because, if everyone tries to do this, no-one will succeed; and there is no way of stopping one's neighbours from trying, except by agreeing not to try oneself. The rational man will recognise this, and will consequently aim at "the best possible life for everyone", simply because this is the only practicable means of securing the best possible life for himself. But one cannot conclude from this, as Baier does, that, even if one could secure one's own interests at the expense of others, it would not be rational to do so.

My own feeling is that, although Baier plays off theory A against theory B, and theory B against theory A, he does not succeed in reconciling them. But one can be grateful to him for making the attempt: both because of the excellence of some of his incidental discussions, and because it is time that someone realised that Hobbism deserves a more convincing answer than it has yet been given.

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D. H. MONRO.

REVIEWS

ENGLISH PHILOSOPHY SINCE 1900. By G. J. Warnock. (Home University Library). London, Oxford University Press, 1958. 180 p. 12*s.* 6*d.* (Australian).

Considered as what it is, rather than as what its title announces it to be, this is a book with considerable merits: it is an eminently clear, accurate and lively account of recent Oxford philosophy (pp. 94-179 are devoted to Ryle, Strawson, Waismann and Austin), and of such of its sources as are to be discovered in Moore (pp. 12-29), Russell (pp. 30-42), Logical Positivism (pp. 43-51) and Wittgenstein (pp. 62-93). For the rest there is a brief description (pp. 1-11) of Absolute Idealism (described as "The Point of Departure") and a number of "Retrospects", which are mainly devoted to a defence of contemporary Oxford philosophy against certain of its critics.

Mr. Warnock's book is certainly not, then, a history of English philosophy since 1900; his readers will not learn, either from text or bibliography, that pragmatism and realism ever occupied the attention of philosophers, nor that even such contemporarily-influential writers as Popper and Collingwood lived and wrote in England during the last fifty years. One cannot say, even, that Mr. Warnock is describing the emergence of recent philosophy at Oxford, for such a historical essay would have to pay considerable attention to Cook Wilson and his followers, who are here entirely ignored.

What Mr. Warnock has done, rather, is to describe the "big moments" in recent philosophy, the moments which would be selected for special attention by an intelligent but orthodox Oxford undergraduate preparing himself for an examination in philosophy. As a result, his book has a double interest: one can read it either for what it says about Oxford philosophers or as an expression of their historical attitudes — their attitudes, at least, to the recent past. Taken thus, Mr. Warnock's book must be warmly recommended as much the best introduction to contemporary Oxford philosophy, with a particularly useful emphasis on very recent work.

At the same time, it contains a number of *ex parte* statements about philosophy and its history which I should certainly wish to challenge; their general effect is to "play down" the extent to which philosophy has recently been following new paths. In this very "playing-down", of course, Mr. Warnock's work is representative of recent tendencies in Oxford philosophy, which now seeks to be accounted respectable rather than revolutionary, but he will certainly mislead the general reader.

The first such statement appears in the preface: "Most philosophy in English has been, for better or worse, and I shall not here say which, something vastly unlike most philosophy in other languages. This is not, as it is sometimes thought to be, a modern phenomenon. It has been broadly true for at least a century and a half, perhaps much longer." No doubt, one can so interpret Mr. Warnock's "most" that his statement is roughly true. But he is certainly slurring over the important fact that this tendency to isolationism has materially increased during the last fifty years. At the beginning of the century, Russell learnt much from Peano, Meinong, Frege, and wrote about Bergson; Moore's indebtedness to Brentano is obvious; Mach, Poincaré, Avenarius, Lotze, Renouvier were household words in philosophical circles. Even later, in the nineteen-thirties, Ryle was discussing Husserl and making a close study of Continental positivism. But now no influential English philosopher ever refers to his Continental colleagues — a fact which can only partly be explained by recalling that Hitler drove so many empiricists out of Europe. For it applies as much to French as to German philosophy.

Secondly, and more important, Warnock denies that there is any novelty in the tendency of recent philosophers to touch so little on the traditional "great issues" of morality, politics and religion. He goes so far as to assert that Hume's central philosophical ideas "bear on morality perhaps but on *religion not at all*" (p. 164, my italics), and makes the more general observation that the great philosophers of the past were not all concerned with religious questions, only sometimes with morality, and seldom with politics. The "great philosophers" he specifically mentions are Berkeley, Hume, Bradley, Locke, Hobbes, Descartes and Russell. Yet of these every single one except Descartes and Bradley published important works on religion, politics and ethics; and even Descartes and Bradley had something to say, even if this was not so considerable, on each of these topics. Perhaps one can justify the change that has come over philosophy and philosophers, but it is surely absurd to pretend that the change has not occurred; over the last thirty years philosophers have certainly been at once more insular and narrower in their interests than they have ever been before, explain that phenomenon how we will. And this fact, at least, ought to be made fully clear to the general reader.

JOHN PASSMORE.

WORDS AND IMAGES: A STUDY IN THEOLOGICAL DISCOURSE. By E. L. Mascall. London, Longmans, 1957. 126 p. 15*s*. (Australian).

This short work by the author of *He Who Is* and *Existence and Analogy* is clearly intended as a contribution to current Oxford discussion on religious language. As a prominent member of the group called Metaphysicals, composed of philosophers and theologians who have been working together on these topics, he has taken his full share of the burden, and he cannot be accused, as conservatives in philosophy are habitually accused, of "not understanding what is happening". Nevertheless, he sets out to defend the classical scholastic bastions against his environment. How far he allows himself to be infiltrated is another matter.

His overt criticisms of modern movements in philosophy are not particularly penetrating or original. On "theology and falsification" he is content to fall back on I. M. Crombie's contribution to *New Essays in Philosophical Theology* (reviewed in this *Journal* by J. A. Passmore, Vol. 35, No. 2, August, 1957). On R. B. Braithwaite's inaugural lecture he is firm and sensible, but, while properly insisting on the "obligatory factual affirmations" of any working religion, he does not see far enough into his opponent's position to develop its difficulties dialectically. *"Une religion sans Dieu! Mon Dieu, quelle religion!"* sufficiently summarizes his amazement. The real interest of this work is in the epistemological chapters II and IV, in which the modern error is traced to its source in the neglect of the scholastic distinction between *objectum quod* and *objectum quo*.

Epistemologists from Locke downwards have treated the sense-percept, through which we understand the world, as if it were the world to be understood: "Knowledge is of the agreement and disagreement between ideas." On that basis, if there is a world other than ideas, it is merely inferred from them, and, as Berkeley and Hume pointed out, the inference is both invalid and pointless. Following this line of succession we get Idealism (which killed metaphysics long before Schlick and Ayer) and Logical Positivism (which has solemnly fired its verbal bullets into the corpse). It is no use complaining about modernity unless one is prepared to go behind the 17th century. We do not know ideas; we know things *through* ideas.

With the idealist error goes another: the error of eliminating contemplation. In Aristotle or St. Thomas, contemplation is the crown of the intellectual life. Since the 17th century, reason has been reduced to ratiocination, losing its serenity and becoming a busybody; and has latterly been confined to the operation of rules which it has made for its own convenience. This, too, is the result of mistaking the *quo* for the *quod.*

Carrying his challenge to the source of the trouble, Mascall asserts: (i) that there is a non-sensory element in perception; (ii) that this non-sensory element consists not of inference but of apprehension; (iii) that the sense-object is to be sensed, not understood; what is to be understood is the "intelligible object of which it is a manifestation"; (iv) that, though the trans-sensory object is normally known through the sense-object, it is sometimes apprehended directly in mystical experience. Our summary comment on these propositions is: (i) that they are insufficiently argued (though the author might plead that he has argued them elsewhere) ; (ii) that they have somehow to escape from the difficulties of representative perception, which are not peculiar to the 17th century; and (iii) that some credit might have been given to at least one modern writer, John Cook Wilson, who in Mascall's own Oxford tried for years to formulate a logic of apprehension in opposition to the Idealist logic of judgment.

With the appeal from logic-chopping to contemplation the reviewer has every sympathy; but the sharp distinction which Mascall makes between trans-sensory cognition and knowledge by inference leads him into difficulties which more prudent Thomists have avoided: it is precisely on such inference that Thomists rely in their proofs of the existence of God. However, we find Mascall, reputedly an Anglican Thomist, borrowing from Don Illtyd Trethowan, a non-Thomist Catholic, his doctrine of "contuition" (p. 85). To contuit is "to apprehend the presence of the cause in a perceived effect"· that is to say, proofs are not needed; in contuition cause and effect are present simultaneously. The purpose of the proofs is in that case not to win assent for a logical demonstration, but to put the hearer in the frame of mind in which the contuition shall occur. It would be interesting to have the comments on this move of an orthodox Thomist reviewer: it is to be suspected that he would respectfully raise his eyebrows.

Assuming, however, that there is "contuition" of sense-objects and their originals, or apprehension of the originals through sense-objects, the question arises: do we know the originals clearly and infallibly? Mascall's answer is, unequivocally, No. He is just as much opposed to "clear and distinct ideas" as to the confusion of idea and object. "My concern", he says (p. 82), "is to emphasize the essentially obscure nature of our apprehension of trans-sensible entities in the act of contemplative penetration." This sentence is written in connexion with our knowledge of God; but it is expressed in general terms, and may be taken to apply to trans-sensory knowledge of any kind. Contemplation, we are told elsewhere (p. 81) is not a seeing, but a wondering; "not a wondering *how,* or wondering *whether,* but wondering *at*". But this emphasis, while it does justice to the experience of the mystics, surely weakens the force of the word "knowing". We can no longer be said to know the thing by means of the percept; we merely peer into the hinterland of the percept with the aid of a fog-lamp. This is a conclusion which need not daunt the natural theologian, for he makes use of rational inference to see him through the fog to the summit. But it is awkward for a contuitionist, for all he can contuit with the percept is an obscuration.

It might seem that at that point faith must take over; and that would be a serious matter for Mascall, for, at that point, natural theology has not even started. He does his best in a tight spot by insisting that knowledge has nothing to do with clarity. He attacks the "dominant school of modern Anglo-Saxon philosophy" for its "over-anxious determination to avoid the possibility of error even at the cost of the quite ruthless impoverishment of the range of human knowledge" (p. 122), and he commends the exploration of the central mystery through a series of analogies (p. 97). But, if this is what knowledge amounts to, it is not directed to an "intelligible object of which the sense-percept is a manifestation", unless an intelligible object is defined as one which we can never understand.[1] And peering with a fog-lamp is not what is commonly meant by contemplation.

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[1] It occurs to me at the last moment that it is just this paradox which Mascall is trying to convey.

There is, no doubt, much to be said for a break between metaphysical inquiry and the sterile quest for certainty; and what Mascall has to say about the element of wonder and mystery in philosophy deserves a bearing in this age of "detachment, ratiocination, and attention to phenomena" (p. 73). But in that case it is misleading to talk about "intelligible objects" and still more misleading to talk about "contuition", for we cannot contuit two objects one of which is hidden from us.

Enough has been said to show that Mascall is, for a Neo-mediaeval thinker, unusually anti-rationalist, and the impression is confirmed in the final chapter on "Knowledge and Communication", in which he accepts, and enlarges upon, Austin Farrer's doctrine of the role of images in theological discourse. This is, briefly, that in the Bible, the source from which Christian theology flows, "the typical instrument of communication is not the concept but the image" (p. 109), and that "images cannot fulfil their proper epistemological function if they are approached in a merely rationalistic spirit" (p. 115). Not only so: "for the understanding of images it is not necessary to get behind them to a non-metaphorical understanding of fact" (p. 116). In theology, in fact, truth is communicated through images, which are drawn directly from the world of sense without conceptual intervention. This is as may be: personally I should have thought that the images used in theology were firmly anchored to affirmations about the state of the case. But, letting that pass, we are not provided with what Mascall seeks to afford us, "a rational justification for the activities of thinking and talking about God" (p. 121). We are provided with a philosophy in which reason is a faculty for assembling disparate and fugitive glimpses of what we at least cannot know,. and a theology which cheerfully by-passes concepts altogether.

It is this discrepancy between the objective and the fulfilment which is disappointing, and it shows how difficult it is even for the most independently disposed to stand out against their intellectual environment, especially in a residential University. Mascall sets out with the best intention, of correcting a contemporary slant to linguisticism and phenomenalism, and makes some admirably pointed and pertinent comment upon it; but he gives way, at a higher level, to just the kind of theologico-philosophical thinking which linguisticists and phenomenalists can make terms with. It is all the more confusing (though also the more creditable) in that he does not want to make terms with them, and believes himself to have made a pertinent point against them. And so, in patches, as we have seen, he has.

But in identifying the intelligible with the obscure he has involved himself in a reverent agnosticism which supplements, without contradicting, modern Oxford philosophy, and from which only the authoritative definitions of his faith can bring him home.

It would be a mistake to give the impression that the book is unimportant. It is full of interesting suggestions, and, as an example of thinking against the current, most revealing. But it cannot have it both ways. Unless the *objectum quod* is somehow accessible to understanding, the accessory concept of contuition is of no avail. To introduce it as a substitute for the classical proofs, even under the guise of interpreting them, is to render it powerless. It may be that there are better ways of arguing the matter than those of Aristotle and St. Thomas — I think there are. But, given Mascall's chosen terms of reference, he has only one option: on to the full Thomist rationalism, or back to the fideism of his Christian colleagues in Oxford.

A. BOYCE GIBSON.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

ADLER, Alfred. The education of the individual. New York, Philosophical Library, 1958. 143 p. $3.50.

"The teacher, professional or amateur, must teach the simple thing: love, and call it by its simple name: love." — p. 114. He must also free the individual from "the classical concepts of space and time", and train him to "live his life as an eternal moment". The author is not the celebrated psychologist of the same name.

APPELBAUM, Bernard. Dimensions of transference in psychotherapy. (University of California Publications, Personality Assessment and Research, No 2.) Berkeley and Los Angeles, University of California Press, 1958. 90 p. $1.75 Paper covers.

ARISTOTLE. On poetry and style. Translated, with an introduction, by G. M. A. Grube. New York, Liberal Arts Press, 1958. 80*c*. Paper covers.

ASOKA. Edicts; edited and translated by N. A. Nikam and Richard McKeon. University of Chicago Press, 1958. xxvii, 69 p. $1.75.

The moral doctrine of "Dharma", as proclaimed by the Indian emperor Asoka in the third century B.C.

BASSON, A. H. David Hume. London, Penguin Books, 1958. 183 p. 3*s*. 6*d*. (U.K.) Paper covers.

A critical exposition of Hume's views on epistemology, morals and religion.

BELL, Hermon F. Talks on religion. New York, Philosophical Library, 1958. 73 p. $3.

BERGSON, Henri. The world of dreams. New York, Philosophical Library, 1958. 58 p. $2.75.

A new translation, by Wade Baskin, of a lecture given before the Institut Psychologique in 1901.

COONEY, Timothy. Ultimate desires New York, Philosophical Library, 1958. 100 p. $2.75.

"Our conclusion in this book is that the ultimate goal of life is Omniman . . . The ought of life is to continually advance the UPD (understanding-power-desire) curve" p. 95.

DRAKE, Henry L. The people's Plato. New York, Philosophical Library, 1958. xxiv, 633 p. $7.50.

A "complete reorganisation of the *Dialogues*" (in Jowett's translation) so that related passages are put together. For example, the section headed "Discovery of the Good Life" begins with pp. 27-38, 48-53, and 40-44 of the *Timaeus*, goes on with pp. 268-277 of the *Statesman*, pp. 245-256 of the *Phædrus*, and so on. The author gives his own comments in the manner of a *compère*.

FINDLAY, J. N. Hegel: a re-examination. London, Allen & Unwin Ltd., 1958. 372 p. 30*s*. (U.K.)
An interpretation which stresses Hegel's connexion with certain contemporary philosophical ideas and methods, but avoids argument with such other interpreters as Stace, Mure and Hyppolite.

FORDHAM, Michael. The objective psyche. London, Routledge and Kegan Paul, 1958. viii, 214 p. 25*s* (U.K.)
Discusses Jung's theories and their application to psychiatry, anthropology and religious mysticism

HANSON, N. R Patterns of discovery; an inquiry into the conceptual foundations of Science. Cambridge University Press, 1958. ix, 241 p. 30*s*. (U.K.)
Examines the "philosophical aspects of microphysical thinking". The author holds that microphysics has often been misrepresented by philosophers because "they have regarded as paradigms of physical inquiry not unsettled, dynamic, research sciences like microphysics, but finished systems. planetary mechanics, optics, electromagnetism and classical thermodynamics" (Introduction). The discussion is therefore concerned with the logic of "growing research disciplines" and includes much of philosophical interest on such topics as observation, facts, causality and theories

JOUVENEL, Bertrand de. Sovereignty; an inquiry into the political good Translated by J. F. Huntington Cambridge, University Press, 1957 xiv, 320 p. 27*s*. 6*d*. (U.K.)
"An attempt to supply a moral and philosophical basis for liberal institutions." Blurb.

KNELLER, George F. Existentialism and education. New York, Philosophical Library, 1958. xi, 170 p. $3.75.
Brief popular exposition of existentialism and its bearing on "modern" educational ideals, which the author regards as fighting against "the forces of depersonalization".

KOESTLER, Arthur. The sleepwalkers; a history of man's changing vision of the universe. London, Hutchinson, 1959 624 p. 25*s*. (U K.)
A history of cosmology, with emphasis on the relation between science and religion.

KUNITZ, Milton K ed. A modern introduction to ethics· readings from classical and contemporary sources Glencoe, Ill., Free Press, 1958. viii, 657 p $7 50
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"In the presentation of this ideology, meticulous care has been exercised in avoiding the impression of a directed agnostication; but more conversely, a firm and vigilant approach has been sought to instil within the reader an awareness of two distinctions evolutionally transpiring in the affixation of the mind and matter interrelationships relative to their structure". Preface.

PASCH, Alan. Experience and the analytic: a reconsideration of empiricism. Chicago, University Press, 1958. xvii, 275 p. $6.50.
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English translation of *Logik der Forschung,* with about 150 pages of new material, in the form of appendices.

RITCHIE, A. D. Studies in the history and methods of the sciences. Edinburgh, University Press, 1958. vi, 230 p. 12*s.* 6*d.* (U.K.)
Chapters on the development of geometry, astronomy, chemistry, biology, and sociology, with the emphasis on the relation between facts and theory. "There is no one universal scientific method, any more than there is one universal method of catching fish. Each fish has to be caught by a method appropriate to that fish" (p. 7).

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NOTES AND NEWS

A.A.P. ANNUAL CONGRESS, 1959

The Annual Congress for 1959 will be held at Adelaide from August 21st to August 26th. The Annual General Meeting will take place during the Congress. Accommodation will be available at St. Ann's College, Adelaide.

The Secretary of the Congress is Mr. C. F. Presley, Philosophy Department, The University of Adelaide, Adelaide, S.A.

JOHN ANDERSON TESTIMONIAL FUND

A fund has been opened to commemorate the work of Professor John Anderson on his retirement from the Challis Chair of Philosophy in the University of Sydney. Professor Anderson, who has several times been President of the Australasian Association of Philosophy, was Editor of this *Journal* for twelve years (1935-1946). He has made outstanding contributions not only to Philosophy but to the intellectual life of Australia. The memorial is to take two forms:

(1) The publication, as a book, of Anderson's major philosophical articles;

(2) A portrait of Professor Anderson, to be painted by Mr. William Dobell and presented to the University.

Contributions are invited from readers of this *Journal,* which owes much to Professor Anderson.

Subscriptions should be addressed to "The Accountant, The University of Sydney, Sydney, N.S.W." and cheques etc., should be made payable to "The University of Sydney". All donations of £1 and upwards will be deductible for the purpose of Australian income tax.

NOTICE TO MEMBERS IN THE U.S.A.

Mr. Alan Donagan has kindly undertaken to act as the United States representative of the Australasian Association of Philosophy, membership of which is the cheapest way of subscribing to this *Journal.* Persons in the U.S.A. may join the Association through him. No money, however, should be sent to him.

His address is: Mr. Alan Donagan, College of Science, Literature, and the Arts, University of Minnesota, Minneapolis 14, U.S.A.

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ARITHMETIC AND REALITY

By HECTOR NERI CASTANEDA

I propose to discuss whether "queer" arithmetics (to be thought of on the analogy with non-Euclidean geometries) can be effectively applied to describe the world. Since this question has been investigated at length and quite recently by Professor Douglas Gasking in his very famous and influential paper "Mathematics and the World"[1], the first part of my discussion is devoted to an examination of Gasking's views and arguments.

I

A Conventionalistic View of Arithmetic

1. *Gasking's view.* Professor Gasking has argued most persuasively for the view that mathematical propositions are like conventions or rules as to how we should describe what happens in the world. The proposition "$7+5=12$", e.g., "does *lay it* down, so to speak, that *if* on counting $7+5$ you do get 11, you are to describe what has happened in some such manner as this: *Either* 'I have made a mistake in my counting' *or* 'Someone has played a practical joke and abstracted one of the objects when I was not looking' *or* 'Two of the objects have coalesced' *or* 'One of the objects has disappeared', etc." (p. 209).

Gasking's argument is non-technical and seems very clear and, therefore, very convincing. As far as I can see his view may be summarized in the following theses:

(A) Mathematical propositions are incorrigible (p. 207, etc.);

(B) They are not descriptive of the world (pp. 208 ff);

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[1] This *Journal*, 1940; reprinted in Flew, *Logic and Language*, 2nd series (Oxford: Basil Blackwell, 1953), pp 204-221. Three-figured numbers in parentheses indicate pages from the latter

(C) Their truth depends on the usage of symbols only (p. 217) but (C1) they are not simply rules for the manipulation of symbols (p. 210);

(D) their characteristic function is to formulate a way of calculating (p. 219);

(E) such a way of calculating together with a way of counting and a way of measuring constitute criteria for our descriptions of the world, i.e., arithmetical propositions are some of the rules governing our describing of reality (p. 219);

(F) there is no necessity in the boundaries separating calculations, countings, measurements, and facts; we can very well draw different contours for, say, arithmetic provided we modify accordingly our countings or measurements or our descriptions of fact (pp. 219 ff).

Of course, it is unfair to represent Gasking as merely making these seven assertions. The point of his paper is to explain them as well as to argue for them. Thus, he argues to (B) from (A), and spends one third of the essay in proving (F), from which he infers (C). He speaks of queer arithmetics for what I have called different contours of arithmetic (cf. Part II below). He discusses the example of an arithmetic in which "$3 \times 4 = 24$", and argues that we could employ it quite successfully in our ordinary problems like paving a floor.

It should be said that Gasking makes claims about mathematical propositions in general, even though his actual discussion is limited to "$3 \times 4 = 12$", "$3 \times 4 = 24$", "$7 + 5 = 12$".

2. *Incorrigibility.* Gasking defines: "A corrigible proposition is one that you would withdraw and admit to be false if certain things happened in the world" (208). And most people would agree with him that mathematical propositions are incorrigible in that sense. Clearly, "$7 + 5 = 12$" tells us nothing about events, so that "no future happening whatsoever would ever prove the proposition false, or cause anyone to withdraw it" (207).

Then Gasking goes on to say that only corrigible propositions can be informative or descriptive of the world. But he seems to assume a definition of 'informative' or 'descriptive of the world' in terms of corrigibility: "A corrigible proposition . . . therefore gives you the information that *those* things (i.e., those things which would make you withdraw your proposition if they happened) *will* not happen" (208). Certainly, if that is what 'descriptive of the world' means, mathematical propositions are absolutely uninformative or nondescriptive. However, the term 'descriptive of the world' is often used in another sense, in which to say that incorrigible propositions or statements are not descriptive of the world is, among other things, to beg the question against Kant, whether there is a synthetic *a priori* or not. In fact, Gasking does continue his argument, and his defence of thesis (F) does seem like a discussion of whether arithmetical propositions can be incorrigible and synthetic, or, as he says, whether or not they "depend on the nature of reality" (210, 212, etc.).

Of course, it is not a good reply that a question is begged against Kant. Kant has been dead for close to 160 years and science, as well as philosophy, has changed tremendously since then. The point is that there are good reasons not to identify corrigibility with being descriptive of the world in the sense of "depending on the nature of reality". Just to mention two cases, we have propositions like (1) and (2) below which somewhat resist such an identification:

(1) One ought legally (i.e., according to the law) to drive on the right hand side of the road.

(2) I have a mild eye-ache.

Proposition (1) is corrigible, but not descriptive of the world; (2) is both incorrigible and descriptive of the world.

When a person says it is illegal not to drive on the right, which is the same as (1), he could be led to withdraw his statement by finding out that the existing law requires driving on the left. Many facts enter into the truth of a proposition like (1); that there is an authority capable of legislating on such matters, that there is a procedure which has in reality been followed, etc. But (1) does not inform us of that, except in the sense that its truth *presupposes* those facts. (1) is a statement about legal obligation, whose simple denial is not the disjunction of all the statements denying those facts, but the proposition that it is not illegal not to drive on the right.

Gasking's own fellow-Wittgensteinians have been arguing that statements like (2) are not descriptive, just because they are incorrigible. They want to take them as highly developed signs of pain; but they are too well acquainted with human nature to deny that utterances like (2) can be true or false. Or when they want to deny that (2) can be descriptive or formulate knowledge and, hence, withhold the use of the adjectives 'true' and 'false' from (2), they have to fall back on "the *sincere* (or *honest*) utterance of . . . ". Obviously, statements like (2) do not fail to be descriptive of the world in the sense that their truth "depends on no fact about the world, other than some fact about the usage of symbols" (217). Furthermore, it will never do to accept that other persons can describe my states of pain because they see and hear me, whereas *I* who feel the pain cannot describe them. Surely, (2) may and does in fact function as a sign of pain to my hearers; but there is no reason to suppose that because (2) functions as surrogate pain behaviour *as well* it no longer serves to describe a mental state of myself.

At any rate, (2) shows that Gasking's assimilation of incorrigibility to uninformativity is far from obvious.

3. *Arithmetic and Life.* Gasking's final argument for the view that mathematical propositions are not descriptive of the world and depend on the usage of symbols hinges entirely on his claim that "we could use *any* mathematical *rules* we liked, and still get on perfectly well in the business of life" (213, my italics in 'rules'). His argument is roughly divided into two parts to show that:

(F.1) We could use any mathematics and compensate for deviations from the present one by means of an adequate technique of counting or measuring (213 ff);

(F.2) We could use any mathematics different from the present one and modify our physics (215-219).

3.1. *Rules.* Before examining the details of Gasking's argument it is important to note that he first speaks of "mathematical rules" and then of "mathematical propositions", as if the two expressions were synonymous. But, although we may say that rules are a species of proposition, not all propositions could be said to be rules. On the other hand, it is natural to say that a rule is not descriptive, but prescriptive; so that Gasking could at least *prima facie* be accused of begging the question.

Indeed, the typical propositions or statements of which we can say that we can use different ones and yet get on in the business of life are *rules,* particularly rules like

(1) It is unlawful to drive on the left-hand side of the road. We could certainly have a different law, making it unlawful to drive on the right, and life might be just as pleasant (or unpleasant) as it now is. However, it is not clear that arithmetical propositions like "$7 \times 5 = 35$" or "$7 + 5 = 12$" are rules. There is not a single bit of ground for saying that these

are rules governing the describing of the world in the way in which (1) is a rule governing the traffic of vehicles on the road.

For one thing, rules tell us what to do or what to avoid doing, but if a storm fells 7 trees and then 5 trees it has felled 12 trees regardless of whether there are human beings or not. No action is prescribed to anybody by the proposition "7 trees and 5 different trees are 12 trees".

Secondly, rules are formulated in sentences containing a "rulish" or prescriptive term like 'ought', 'forbidden,' 'required', 'illegal', 'unlawful', 'permitted', etc. It is not easy to find a place for such terms in "$7 + 5 = 12$".

There are indeed mathematical rules like "To multiply a whole number by 10^n you may (are permitted to) write n zeros to the right of that number". There are also rules for the application of mathematics like "To find out how many tiles, each 1 foot square, you need to pave a room 20×50 feet, you ought to multiply 20×50". But the important thing is that all such rules are derived from mathematical propositions asserting a given relationship between numbers. The rules are, in Kant's terminology, hypothetical imperatives; they follow from the non-rulish propositions, by means of the ordinary logic of rules or norms[2]. Similarly, once we have the proposition "$7 + 5 = 12$" we have the ground for the rule "If you are discussing the number which is the sum of 5 and 7, you may discuss instead the number 12", or a ground for the rule "If you put 7 apples in the basket and then 5, you may say that you put 12".

In any case, it is worth noticing that rules, i.e., real, ordinary rules like "It is not permissible to drive on the left", may be said to be:

i) corrigible — the legislators might not have enacted such a law;
ii) synthetic — in the sense that they could be disobeyed;
iii) not empirical — they are not discovered by observation;
iv) not descriptive of the world — but prescriptive;
v) not dependent on the usage of symbols only — enactment is necessary;
vi) replaceable — i.e., using different, incompatible rules we could get along in the world.

It is not difficult to see that Gasking's view of mathematical propositions as rules lets him hold that mathematical propositions are replaceable (feature vi), not empirical (feature iii), and not descriptive of the world (feature iv). One can see that the underlying movement of Gasking's argument takes him from the incorrigibility of mathematical statements to their non-empirical character, and then, since he assimilates non-empirical to non-descriptive (pp. 206 f., 219 bottom), the dichotomy "either descriptive or rulish" forces him to classify them as rules.

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[2] Cf., e.g., H. N. Castaneda, "Nota sobre la logica de los fines y medios", *Universidad de San Carlos* (Guatemala), No. XXIX (1956); "Imperative Inference", *Philosophy and Phenomenological Research* (*forthcoming*).

However, once we reflect on the nature of rules it is clear that Gasking is now heading for trouble. At the bottom of the replaceability of ordinary rules lies their corrigibility. Surely, to say that the rule may be to drive on the left rather than the right is in part to say that it might have been *false* that we ought to drive on the right. Likewise, the possibility of a queer mathematics in which "$7 \times 5 = 75$" entails that "$7 \times 5 = 35$" *could* be false, i.e., that it is corrigible. Therefore, Gasking's argument has the perplexing characteristic that, even though he identifies corrigibility with being descriptive of the world, his strongest step involves showing that ordinary arithmetical propositions could be falsified *and so* are not descriptive of the world!

Clearly, a proposition whose truth depends on linguistic use alone, like "Bachelors are unmarried", cannot be falsified *at all*. We could drop the word 'bachelor' and use 'solor' in its stead, but the proposition "Solors are unmarried" is exactly the same, and continues to be unfalsifiable. We may stop using words with which we could say that bachelors are unmarried, but that is not to use a queer system of family relationships.

I do not know of any way of repairing Gasking's argument. If mathematical propositions were rules he could defend their replaceability, without having to bother with whether they are corrigible or not. But I have indicated some reasons which make it difficult to disguise them as rules. I pass over the view that analytic propositions are rules and that, since mathematical propositions are analytic, they must be rules. This view begs the very question we are discussing: whether or not mathematical propositions are descriptive of the world, i.e., non-analytic[8]. However, a view can be illuminating even though it is false and the arguments for it are invalid.

3.2. *Queer Countings and Measurings.* Gasking argues that we could employ a queer arithmetic in which $6 \times 4 = 12$ and get on perfectly well with the business of life. He does not define a queer arithmetic, but it is clear from his discussion that he does *not* mean one or more changes of labels. In other words, to use the formula "$6 \times 4 = 12$" is not merely to use '6' to mean the number 3, and perhaps '3' or 'vi' to mean the number 6. These are trivial changes in numerical terminology. Obviously, such changes are present in our daily experience, as when we translate from English into German, or as when a person translates numbers into the binary system to make a computer work some problem. By a 'queer arithmetic' Gasking means something more exciting. Presumably, we are to think of queer arithmetics on the analogy with non-Euclidean geometries. Provisionally, just to fix the sense of the argument, we may say that a *queer arithmetic* is a system of propositions about natural numbers in which we have a different multiplication table. (This topic will be given an independent and detailed discussion in Part II of this paper.)

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[8] Besides, the major premise is false. For a thorough discussion of the view that analytic propositions are rules, *cf.* A. Pap, *Semantics and Necessary Truth*, (New Haven: Yale University Press, 1958), 170 ff, 182-5.

Gasking says that in that queer arithmetic the product $m \times n$ is the number that we obtain by the formula: $(m+2)(n+2)/4$. (Let us not ask now about the nature of the translation in question.) He argues that by means of a different "technique of measuring lengths" (214) a builder could pave a room 6 yards long and 4 yards wide:

> "He lays his yardstick along the longer side, with the '0' mark of the yardstick in the corner, and the other end of the stick, marked '36 inches', some distance along . . . he counts 'one'. He then pivots the yardstick on the 36 inches mark, and swings it round through two right angles, till it is once more lying along the side of the room — this time with the '36 inches' mark nearer to the corner . . . he counts 'two' . . . he pivots the stick about its middle and swings it round . . . he counts 'three'. He then swings the stick round through two right angles, pivoting on the '36' mark, counting 'four'. He then reverses its direction, as before, counting 'five' . . . " (213 f.)

Clearly, if he were to count the tiles, after he paved the floor, the builder (hereafter to be called "Gaskon") would have to count: 'one' for the first tile, 'two-three' for the second, 'four' for the third if it is the last, or 'four-five' if it is not the last, etc. Hence, the "queer technique" of measuring requires a similarly "queer technique" of counting. (This, incidentally, shows that measuring and counting are not independent concepts or operations, as Gasking implies on p. 215.) Counting is, in fact, the simplest form of measuring, and every measuring includes it or presupposes it.

It is important to note that Gasking's talk of a "different technique" is deceptive. He is dealing with a different concept of counting and measuring. Different techniques under the same ordinary concept of counting are, e.g., 1) counting by saying 'one' 'two', 'three', etc. to different objects, 2) counting by pairs or fives, 3) weighing the totality of objects, weighing one of them, and dividing the two numbers (when the objects involved are supposed to have the same weight), etc.

In Part II, I shall show that "queer" countings either lead to contradictions or involve at most an infinity of trivial changes in numerical terminology. In the present case we may notice in passing that Gaskon is supposed to count twelve tiles as we do, in order to pave the floor. But once he has paved the room he will *have to* count in the queer way he measures to speak of 4 and 6 tiles or squares on each side, as illustrated:

	1	2–3	4–5	6
1	1	2–3	4–5	6–7
2–3	8–9	10–11	12–13	14–15
4	16–17	18–19	20–21	22

$6 \times 4 = 12$

Gaskon must count abnormally to find that the room is 6 yards long and 4 yards wide; but then he will find that he needs 22 tiles, whereas, according to his arithmetic, he needs only 12.

We should also notice that in such a queer arithmetic, in which we are supposed to have all natural numbers, we cannot employ the usual rules of addition: "2 tiles and 1 tile = 4 tiles"; on the other hand, " 3 tiles and 1 tile" has no meaning. But let us postpone the development of these matters.

One can easily see that Gasking's argument looks persuasive only because he has isolated 1) the measuring of the room from the counting of the tiles or squares, and 2) multiplication from the remaining body of arithmetic. One suspects that, just as it is impossible to isolate measuring from counting, it is not possible to isolate both counting and measuring from the properties of physical objects. That is to say, all the cases that Gasking discusses are probably in the end instances of his last case: that, by changing the laws of physics, we might very well get on in the business of life with a queer arithmetic. Thus, his last argument is the fundamental one.

3.3. *Queer Physics.* Gasking's more dramatic claim is that Gaskon could count and measure the way we do, but multiply according to different tables, and yet get exactly the right number of tiles to pave his room (217). He offers both a general argument and an illustration. I quote the former in full:

> "Imagine that we did use a 'queer' arithmetic, in which $3 \times 4 = 24$ . . . if we did use this queer system, we should stick to the proposition '$3 \times 4 = 24$' no matter *what* happened, and ascribe any failure of prediction of builders' requirements, and so on, *always* to a mistaken view of the physical laws that apply to the world, and *never* to the untruth of the formula '$3 \times 4 = 24$'. This latter proposition, if it *were* part of our mathematical system, would be *incorrigible,* exactly as '$3 \times 4 = 12$' is to us now." (217.)

He then goes on to illustrate. When he sees that 12 tiles are enough to pave the room, Gaskon would say, e.g., that "the tiles must expand to double their area on being put into the room" (218).

Of course, Gasking is right in everything he says here. Since mathematical propositions are incorrigible, *if* "Napoleon weighed 50 pounds at birth" were a mathematical proposition it would be incorrigible. This contrary-to-fact conditional is logically true. But from it we can infer neither (a) that "Napoleon weighed 50 pounds at birth" is a mathematical proposition, nor (b) that it is incorrigible, nor (c) that it is a legitimate candidate as a mathematical truth. Gasking's argument is simply an *ignoratio elenchi.* The issue is neither whether "$3 \times 4 = 24$" would be incorrigible, nor what we would say if certain facts did not come out right when we used that formula for prediction. The issue is whether the formula is a legitimate alternative in arithmetic — in the way in which the proposition in Riemannian geometry that there are no parallels is or is supposed to be a legitimate alternative to Euclid's postulate.

This argument also brings into focus the perplexing feature mentioned in section 3.1. If "$3 \times 4 = 12$" is absolutely incorrigible, and this is the assumption from which Gasking is arguing, *nothing at all* could make it false, regardless of how much the description of the facts were to be changed. The only possibility would be, as was the case with the analytic proposition "bachelors are unmarried", to make a trivial terminological change—but this has been excluded by the definition of a queer arithmetic.

Even Gasking's illustration looks more plausible thanks to his method of isolating propositions and cases. For us to say meaningfully that the tiles double in size when brought into the room, they must be smaller outside the room; moreover, things inside the room must have the same constant size, everything else being equal. As Gasking correctly emphasizes:

> "In this case we find that the tiles expand by *measuring, counting and calculating in a certain way* — by finding that the tiles each measure 1 × 1, that the room measures 3 × 4, or 24 square yards, and that we can only get 12 tiles in it. This is our sole *criterion* for saying that the tiles expand. That the tiles expand *follows from* our queer arithmetic." (219; his italics.)

Let us suppose now that Gaskon draws a rectangle like the floor of the room outside the room and tries to pave it with the same tiles:

	1	2	3	4
1	1	2	3	4
2	5	6	7	8
3	9	10	11	12

3 × 4 = 24; yet 12 tiles are enough

Then, inside the room, he cuts a piece of paper into three strips and each strip into four pieces. According to his "multiplication" table he should have 24. He counts the pieces of paper and finds only 12. Thus, it seems that Gaskon should say that things expand: (a) when they are brought into the room, (b) when they are taken out of the room, (c) when they are left in the room, and (d) when they are left outside the room. In other words, there is no expansion: from what size did they expand? So Gaskon can say that from his queer arithmetic it follows that the tiles *cannot* expand.

But now Gaskon has become possessed of the scientific demon. He counts 12 tiles, then he makes 4 piles of three each, so that according to his queer arithmetical proposition "3 × 4 = 24" he has to say that they have doubled. He counts "one, two, three; four, five six; seven, eight, nine; ten, eleven, and twelve", so that the additional ones vanish; but since the piles have remained intact he has both 12 and 24 tiles — yet 12 and 24 are different numbers!

Gasking may say that there is no contradiction, for there are two different operations involved, and if we distinguish them everything is all right. Well and good; but then Gaskon is bound to say that the operation '×' cannot be an abbreviation for counting, and that the only way of determining how many tiles he needs to pave the room is to measure the floor by counting the squares, as in the diagram above. He may eventually invent ordinary multiplication as a shorthand for addition. In other words, the operation Gasking represents by '×' is *either* the source of contradictions if both it and ordinary multiplication are taken to be the same operation, or it is an altogether different operation which can peacefully coexist with ordinary multiplication. In the latter case Gasking is not discussing a queer arithmetic, but only ordinary arithmetical operations which (i) are not shortcuts for counting, (ii) are correctly performed on numbers without contradiction, (iii) are not involved in counting and (iv) are such that the propositions formulating them are *already* incorrigible, like any other arithmetical proposition.

In other words, if Gaskon's 'queer' arithmetic involves no contradiction, it is only a terminologically different arithmetic, in which the operation $\neq$, defined by "$m \neq n = 2 (m \times n)$", has been called "multiplication" and the sign '×' has been used to supplant '$\neq$'—in spite of the resulting ambiguity!

I conclude, therefore, that Gasking has not proved his most important claims, (B) that ordinary arithmetical propositions are not descriptive of the world, (C) that they depend on linguistic usage only and (F) that queer arithmetics could very well be applied to reality. I have pointed out a conflict between his theses (B), (C) and (F). So perhaps one of them may after all be true. Even though I have attacked Gasking's arguments for (F), it is not completely clear from my discussion that (B) and (C) are true. For one thing, all three may very well be false. In any case, I propose to examine thesis (F) on its own merits.

II

Alternative Arithmetics

1. *Queer Arithmetics.* Let us follow Gasking in the use of the term 'queer arithmetic'. But we must give it a precise meaning if we are to get somewhere. The following definition seems to me to be adequate for a general discussion of Gasking's problem:

Def. A *queer arithmetic* is a system of propositions about numbers which includes at least one proposition incompatible with the propositions proven in arithmetical treatises and manuals.

It should be noticed that:

a) Peano's postulates alone do not constitute a queer arithmetic, but only an incomplete formulation of arithmetic. Those postulates (to be discussed below) formulate formal properties of the sequence of natural numbers, but do not tell us how numbers are used in counting and measuring.

b) There are trivial changes that can be introduced in the numerals, which cannot produce a queer arithmetic. One such trivial change would be to use the sign '4' to stand for the number 3, and either the sign '3' or another sign to stand for the number 4. Clearly an infinite collection of such trivial changes does not yield a queer arithmetic. We may speak of a ***trivially different arithmetic.*** One such example would be to use the signs in the first row for those just underneath them:

(G1) 1, 2, 4, 6, 8, 10, 12, 14, . . . , 2n–2, . . .
1, 2, 3, 4, 5, 6, 7, 8, . . . , n , . . .

(G1) is the trivially different arithmetic involved in Gaskon's queer countings and measurings discussed in Section I.3.2 above.

c) There are (as we have seen) trivial changes in the terminology of arithmetical operations or functions. We may call "multiplication" the operation of doubling the product of two numbers, and we may speak of "semi-multiplication" for ordinary multiplication. Again, these changes may be compounded to infinity. In an excess of clarity we may call the systems obtained by such changes ***trivially changed arithmetics.*** One such arithmetic, we saw in I.3.3. above, misled Gasking.

2. *Ordinary Arithmetic.* The ordinary arithmetic of natural numbers can be characterized by two sets of propositions:

A) *Peano's postulates,* which determine the natural sequence of numbers:

(P1) 1 is a number.

(P2) Every number n has an immediate successor, to be called n', which is also a number.

(P3) If two successors m' and n' are the same number, then m and n are the same number.

(P4) 1 is not the successor of any number.

(P5) Every property P is possessed by all numbers if 1 has P and if, when *n* has it, *n'* has it too.

B) *The principles of counting,* which formulate the basic manner in which natural numbers function in experience:

(C1) To count the objects of a collection or aggregate K is to establish a one-to-one correspondence between the objects of K and a set N of numbers (or numerals), such that:

(C2) N includes 1;

(C3) there is at most one number in N whose immediate successor is not in N;

(C4) the number of objects in K is the number mentioned in (C3) if it exists; otherwise the number of objects in K is infinite;

(C5) the one-to-one correspondence may be carried out (i) by actually attaching one number (or numeral) to each object; or (ii) by forming partial non-overlapping correspondences of type (i), as when we count by twos or fives or hundreds; or (iii) by specifying a rule for actually attaching numerals to as many objects as we please.

Obviously, since, as Peano, Dedekind, etc. have shown, all formal properties of numbers follow from (P1)–(P5), a queer arithmetic worth discussing must be one which differs from ordinary arithmetic in at least one of Peano's axioms or one of the principles of counting. Otherwise the different arithmetic is trivially different or trivially changed, or self-contradictory. Thus, there are at most ten fundamental types of queer arithmetics.

3. *Numbers and the World.* I want to argue in some detail that Gasking's claim that a queer arithmetic could be applied to describe reality is ungrounded. Every queer arithmetic, i.e., one which does not turn out to be either an incomplete or a trivially different or changed arithmetic, is inconsistent or inadequate to help us get on in the world in all situations.

3.1. *Non-Peanian Arithmetics.* To begin with, I shall show that a queer arithmetic which contains a change in Peano's postulates requires a different concept of counting. So if, as in Gasking's final argument, we are to suppose that Gaskon has a queer arithmetic, but his counting and measuring proceed as usual, then Gaskon's computations must produce results contradictory to those which he obtains from counting.

CASE 1. If 1 is not a number we must start counting with two. Thus, when Gaskon says that he needs three tiles he actually needs two, and in general whenever he says "n objects" we can translate "n–1 objects'. Hence, (P1) cannot be the source of a queer arithmetic.

CASE 2. If we abandon (P2), at least some numbers, say 8 and 25, do not have immediate successors, or if they do their successors are not numbers. In the former situation we could not count beyond 8, for (C3) does not allow us to make a new start; aggregates with 10 and 12 objects would be uncountable. This may not be inconsistent, but will certainly make it impossible for Gaskon to pave his room — except by a lucky chance, since he could not have a way of distinguishing 12 tiles from 11 or 14. Of course, this is not the queer arithmetic Gaskon was left with, for he was supposed to know that 11 apples and 12 tiles were exactly what they are for us; he was able to count up to any number. Thus, Gaskon was supposed to be able to do the impossible.

On the other hand, if the sequence of natural numbers is only supposed to have, as it were, a hole, we find a trivially different arithmetic. If 8 has a successor, 9, which is not a number, when we count we are just to bypass 9. Arithmetic is the same as usual, except that after 8 (and before 26) all numbers are moved one place, and after 26 they are moved two places.

CASE 3. If 1 is the successor of, say, 25, (C1) and (C5) require that we stop counting at 25. Thus, abandoning (P3) takes us back to Case 2.

CASE 4. Suppose that 100 and 50 have the same successor, even though they are different. Thus, we may have the following sequence of natural numbers:

$$\begin{tikzcd}
1, 2, \ldots, 50 \arrow[dr, dash] & \\
& 51, 52, 53 \ldots \\
a, \ldots, 100 \arrow[ur, dash] &
\end{tikzcd}$$

According to (C2) we must count "One, two, . . . "; according to (C3) we must say "fifty-one" after saying "fifty", and cannot use "a".

Thus, the segment "a, . . . , 100" is altogether irrelevant, and we obtain the usual arithmetic.

On the other hand, suppose the resulting sequence were:

1, 2, . . . , 50, 51, 52, . . . , 98, 99, 100, 51, . . .

If the sequence repeats in cycles after 50, according to (C1) and (C5) we cannot count beyond 100, and are back to Case 2. If the numbers following the second 51 are different, say, 101, 102, . . . , we are led to contradictions because of the ambiguity of "51 apples". If one 51 is disregarded in counting we obtain the normal arithmetic.

CASE 5. Suppose that there is a property, call it R, which 1 has and if *n* has it *n′* has it too, but not all numbers have it. If only (P5) is false we get a transfinite arithmetic, but transfinite numbers are not used in counting, so our ordinary arithmetic is not queer. If no other numbers transfinite or appendetic like "a, . . . , 100" at the beginning of Case 4, are included, then the negation of (P5) produces inconsistency; for nothing could fail to have R. If besides (P5) (P4) is also false, then a situation like the one described above with the segment "a, . . . , 100" preceding 51 would be possible. But this does not produce a queer arithmetic, only a "larger" arithmetic which includes ordinary arithmetic.

3.2. *Queer Countings.* Suppose now that a queer arithmetic includes both Peano's axioms and different principles of counting. Again, there are at most five basic types of queer arithmetic. I shall show that they either involve self-contradiction or prevent Gaskon from getting on in the business of life.

CASE 1. We give up the one-to-one correspondence, for example:

a) we count "one-two, three-four, five-six, . . . ",
b) we count "one, two-three, four-five-six".
c) Gasking's special technique of counting discussed in 3.2. of Part I: "One" for the first object; we say "two" for the second if it is the last, otherwise "two-three"; we say next "four" or "four-five", depending on whether the object is the last to be counted; and so on.

In some of these "countings" we just use a term like 'one-two' for '1', 'three-four' for '2', etc., so that there is no queer arithmetic, but only a trivially different one. At any rate if we are to avoid contradictions we cannot, e.g., allow that 12 apples for *us* be regarded both as 25 and as 30 apples and say that 25 and 30 are different numbers. We must have a general rule as to how the correspondence between numerals and objects is to be made. Thus, using a queer arithmetic of this sort, we should speak of $\phi(n)$ objects for every situation in which we now say that there are *n* objects. And the counting will have to be "$\phi(1)$, $\phi(2)$, $\phi(3)$, . . . , $\phi(n)$". Now, if the other numbers are disregarded we just have a trivial change in terminology; if we consider that Peano's postulates apply to the ϕ-numbers as well as to the other numbers, we do not produce a queer arithmetic, either. This only shows the well-known fact that Peano's postulates are not a complete or unique characterization of the sequence of natural numbers. Since the natural numbers are those attributed to objects in counting, the ϕ-numbers *are* the natural numbers under a new name.

Let us consider now the correlation of one numeral to several objects. In fact we often do it, as, e.g., when we count pairs or triplets or piles. But suppose we counted: "one (taking or pointing to one object), two (taking two objects), three (taking two), . . . ". We could not count the persons forming a monogamous marriage or the persons forming two such marriages. Clearly, the arithmetic is not queer, but we may find it difficult to determine how many tiles we have to buy for a certain room whose dimensions we cannot ascertain, unless we interpolate "fractions" and come to use the trivially different sequence "1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, . . . ".

CASE 2. If we never start counting with '1', we are only changing the names of the numbers.

CASE 3. If we are not to use certain numerals in counting, they have to be fixed by a rule; otherwise we may fall into contradictions, or provide no way of finding agreement, for we may count from 1 to 15 once and then count the same objects correctly, jumping, say, 8 or 10, and obtain 16 or more objects. Let the numerals to be omitted be fixed. Then we may just have a trivially different arithmetic with terminological changes after those numerals. But we may find that our arithmetic is inadequate or leads to contradiction. Suppose, e.g., that '9' is omitted. Eight apples and 1 apple will be 10 apples. If 8 and 2 are also 10, we get a contradiction; if 8 and 2 are 11 then we can use these addition tables in life, but we have only substituted '10' for '9', '11' for '10', etc.

CASE 4. We may abandon (C4) and take as the number of counted objects one whose successor is also mentioned in the counting operation. This amounts to correlating several objects to a numeral and leads us back to Case 1 above. If we take another number, then we are actually correlating several numerals to one object, which also fails to produce an applicable queer arithmetic.

CASE 5. Obviously contained in the previous cases.

3.3. *Conclusion.* Queer arithmetics in the sense defined cannot apply to the world. Thus, what I have called Gasking's thesis (F) is false, namely, that we are free to use a queer arithmetic provided we introduce certain changes in our technique of counting and measuring or in our description of the facts, i.e., our physics.

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TRUTH AS THE ACHIEVEMENT OF KNOWLEDGE

By Roy Wood Sellars

In this paper I want to give, in outline, what seems to me to be the correct framework for handling the categories of truth and knowledge. I call these epistemological categories. This is in accordance with my contribution to the book, *Essays in Critical Realism* (1920). I shall hold that knowledge is the primary category and that truth is its achievement through the operation of verification. In this context, I speak of beliefs as knowledge-claims. And, as I see it, knowledge has objective import and is concerned with the constitution of *being*. In this fashion, epistemology links up with ontology. Thus, I am a physical realist.

The two categories, knowledge and truth, are very intimately related; and it is because they are so intimately related that confusion occurs. We can put it this way: Knowledge, as an achievement, points outward to its object, while truth points not only to knowledge but to its complex foundation. That is where verification comes in.

I have in mind here, of course, empirical knowledge as it begins at the elementary level of perceiving and advances in everyday life and in science. Perceiving which achieves perceptual knowledge is called veridical. It tells us something, *in a guided way,* about the world around us. I speak here of direct, realistic empiricism and stress guided reference tied up with the pole of response. The functional unit is S-C-R. I break completely with the pre-biological unit of traditional empiricism, S connected with a sensation.

But I must not anticipate too much. To come back to the epistemological categories, I would take the adjective "true" to apply to statements taken as knowledge-claims. So applied, it means "is a case of knowledge and acceptable as such". This framework and terminology I worked out fairly early. It can be found in my first book, *Critical Realism* (1916), chapter 10. Knowledge, as an achievement, *implies* the correspondence, or adequacy, of the statement to its object. Correspondence is not a test but an implication. Here I find that my line of thought connects up with the semantic theory of truth in recent logic. But, as an epistemologist, I must explain this achievement by looking at the foundations of verification; which will take me to a consideration of the whole mechanism of perceiving as a controlled and guided activity of the organism in its world.

All the niceties of present usage can be employed. We can distinguish between sentences and statements and take a statement, as an assertion, to be an objective knowledge-claim. We must, however, take knowledge, as an achievement, to be an ultimate category which *cannot* be reduced but which can be explored. I myself take it to express the disclosing grasp which verified statements have on their referents, that is, broadly speaking, on things and events. It is usual to speak of such disclosures as facts. In knowledge, we have facts about things. And facts are *in re* only in the sense that knowledge is attained. It is here that epistemology links up with ontology. I am accustomed to speak of knowledge as a deciphering of the constitution of being. It is a remarkable achievement with a complex foundation which goes back ultimately to the controlled functioning of the sense-organs.

I shall have something more to say about knowledge in the sequel, when I talk about existence. For the realistic empiricist, knowledge is about things and not about ideas. Thus things are cognitively attained. Hence we can explore knowledge without seeking to reduce it so something else. While *sui generis* as an achievement, the whole machinery of sense organs and cortex, of sensations and concepts, is at its service. We can pass from the *what* to the *how*.

A few historical remarks are in order. American realism reflected dissatisfaction with both idealism and pragmatism. The expression "epistemological monism' was coined by the new realists to characterize their searchlight, or presentational, view of perceiving. As Perry put it, the idea *is* the thing. I coined the expression "epistemological dualism" to mark the rejection of this view of knowing. I held that knowing is an affair of directed reference and characterization. The reference is built up on the response pole of perceiving and carries conceptual characterization with it. It is dualistic in the sense that it rejects the presentationalism of the searchlight and the sensationalistic tradition. It stresses knowing *through* ideas, or statements, referentially directed. It is an affair of aboutness and of conceptual application. But it is a form of direct realism as against Locke and Kant. I would also remark that it concerns itself with physical things and is thus existential in import. This is the reason why to say that this thing before me exists is tautologous. Russell's use of the term "exists" as concerned with the applicability of concepts is, of course, secondary.

Since knowledge is achieved through statements, it follows that these must be *such that* they can convey knowledge. This is their correspondence or adequacy. But it must be tested and verified in operative ways going back to the foundations as well as to its working. As we have said, correspondence is not itself a test but an implication of the fact of knowledge. I brought this point out in both the *Philosophy of Physical Realism* and in my study of Realism in Winn's *American Philosophy*. As I see it, the semantic definition of truth to the effect that the statement, "It is raining" is true if, and only if, it is raining is an application of the above analysis. What more is needed is the understanding of tests and foundations And that is more an affair of epistemology than of logic.

One more introductory point. I have spoken of S-C-R as the functional unit. In man, through language, C has a tremendous development so that response is lifted to communication and designation. Yet it is still the underlying mechanism. I am interested in seeing how linguists and psychologists will work this out. The book entitled *Language, Thought and Culture,* edited by Paul Henle, has interesting material in this connection.

And now I am going back to the first decades of this century when there was something of a stalemate between three classic conceptions of truth, namely, the coherence conception of idealism, which goes back at least to Spinoza; the pragmatic conception, which stressed verification through predictive working, and the correspondence theory. which held that ideas, if true, corresponded to something objective. I leave out the Cartesian appeal to self-evidence, as associated more with intuitional rationalism than with empiricism.

These positions had their setting and their points. Coherence emphasized consistency and inclusiveness and rejected the realistic dimension. Royce should be read in this connection. Both Bradley and Bosanquet were experientialists. Pragmatism was strong in its emphasis upon the temporal process of verification and working but did not dwell upon the import of knowledge-claims. It at least tended to lack the realistic dimension and did not do much with perception. To me it was vague about physical things and such entities as atoms and electrons. If in the James form it was too subjective, it was made more objective by Peirce and Dewey. Here the emphasis was upon the instrumentality of ideas and their acceptance. in the end, as affairs of competent decision. The correspondence tradition remained in the background as supposedly outmoded. Russell urged it but in a varying epistemological setting. As with the later positivists, sense-data stood out as primary. I have a pretty clear memory of the issues of this period. Both idealists and pragmatists rejected correspondence because they could not see how it could be tested. Certainly, it could not be done by comparison.

It was clear that something more basic had to be done. The realists tried to do this. I am going to expand the argument of the critical realists. I do not think it got a proper hearing. Soon there were too many excursions and alarms. There was a shift away from epistemology to proposals about meaning and language. These things have to work themselves out. I have the impression that positivism *qua* phenomenalism is fighting a losing battle. What is now needed is a clear, realistic alternative. I have mentioned the appearance of the semantic definition, which fits in with realism. The only other novelty seems to be the performatory theory of truth which calls attention to various linguistic uses of the word "true", such as that which is equivalent to a nod of agreement. But it neglects verification demands, as J. Kincade points out in the July 1958 number of *Mind*.

It will be remembered that it was the feeling of both the new realists and the critical realists that neither the idealists nor the pragmatists had done justice to the realistic demands of perception. I have the impression that when Dewey moved to Columbia he was somewhat affected by Woodbridge and Montague, both of whom were realists. The older Columbia men could, undoubtedly, give us information about this point.

Now I am not going to say anything much about the new realism, except by way of contrast. The sceptical dictum of the early thirties, as I recall it, was to the effect that the new realists could not account for error and the critical realists could not account for truth. It is to the second half of this dictum that I want to address myself.

In the first place, critical realism was *not* a revival of Lockianism nor of Kantianism, with its things-in-themselves. Rather, it expressed a re-analysis of perceiving, with emphasis upon the pole of response, so that the functional unit was S-R, or better, at the human level, S-C-R; that is, with learning and concepts intercalated between stimulus and response. In the old tradition of the "causal theory of perception", the unit had been the stimulus with its effect, sensation. The more complete biological unit was ignored because it was the age of classical physics and Cartesian dualism. As a consequence of the stimulus, sensations popped up in the soul-mind. Positivists and phenomenalists have just gone one better than this by starting with sense-data. To many young men, it sounds hard-boiled. I have always held that a frank materialism is still more hard-boiled.

Now the new realists were aware of the above shift of framework in the direction of response and behaviourism. I like always to give my opponents their due. Dewey became increasingly behaviouristic as the years went by. But, as I see it, the new realists jumped too hastily into the pan-objectivism of extreme behaviourism with its rejection of consciousness and the subjective. Bergson's view of the brain seems here to have influenced Perry. And it always seemed to me that Dewey fought shy of consciousness and the subjective, handling them in a sort of dialectical fashion. I had the impression that he concentrated his attack on the new realism, especially on its universalization of cognition, and on the essence doctrine of Santayana.

The new realists were afraid of representationalism. Hence their gambit of the searchlight view of perceiving with its rejection of consciousness and the subjective. I, on the other hand, devoted my energies to a basic reconstruction of our ideas of perceiving. I regarded sensations, connected with the stimulus, as guiding perceiving and so emphasized the response pole of the whole activity. At the human level, learning and language intervene to make perceiving an affair of directed designation and characterization. Here is where objective reference gets its base of operations. It is that chair over there which I am looking at and characterizing. I am quite ready to admit all the modifications of atomistic sensationalism needed and to stress constancy and *Gestalten.* But the point is that perceiving is a directed affair and concerns itself with the things around us. Visual perceiving is guided by my visual field but is concerned with what I am looking at.

With my friends, the animal psychologists, I like to trace the development of this framework and see how it flowers at the human level in designation and characterization and in the rise of *categorial meanings,* such as existence, thinghood and causality. Hume could never account for them on his truncated sensationalism and Kant had to introduce them *a priori.* I sketched this handling of the categories in my *Evolutionary Naturalism* (1922).

Now it is obvious that this line of analysis of perceiving confronted the mind-body problem on different terms than did either the new realism or Dewey. It had to face up to it. I leave Montague's twistings and turnings out of the picture. He was less doctrinaire than Perry; and quite aware of the complexities of the problem. But I do not think he had the right epistemological lead.

I developed a double-knowledge approach in which I held guiding sensations to be "natural isolates", in cerebral processes, which could be experienced participatively, while the neurologist could have about the cerebral event only the sort of knowledge which takes its rise from referential perceiving. I do not think that Leibnitz's mill-machinery analogy does justice to this situation.

Sensations can be quite subjective and functional. But perceiving has its referential pole which is objective in import and is enriched by gesture and language. It is at this response-pole that realistic empiricism escapes from the subjectivism which haunted it from its inception and still lingers in phenomenalism. We perceive, refer to, and characterize the things around us. It is in this referential framework that concepts develop and operate. They are tied in with symbols and their use. Here we speak of understanding the meaning of words. There is no doubt but that Wittgenstein has made contributions here.

Perceiving, so taken, is concerned with a common, objective world around which communication can develop. I can point understandingly to the same thing you are pointing to. We are parts of a world and agents in it. The Cartesian gambit fails. Those who, like Moore, start with sense-data, are still trying to prove an external world. I take this to be an artificial problem due to a wrong start.

We are now ready to come back to the question of knowledge and truth. It should be clear that, in this framework, directed knowledge-claims are primary. We have good reason to believe that we are perceiving, that is, denoting and characterizing the very things we are adjusting ourselves to. We do not first know ideas and then *infer* things. We have undercut vicious representationalism. In this context, "true" can be taken to mean "is a case of knowledge". The knowledge-claim is primary. And this signifies the acceptance of *facts* about objects. These facts are the sort of disclosures knowledge can give. They are cases of cognitive attainment. As I have indicated, we can explore knowledge but not reduce it to something else. But we are aware that it is conveyed by statements having a conceptual composition. If knowledge, then the statement must be *such that* it can do the job. Thus correspondence, or adequacy, is an implication of the fact of knowledge.

But statements must be subject to test before they are acceptable as giving knowledge. Here is where the category of truth enters. It is Janus-faced and points to knowledge by way of verification. And this can be only by way of tests. And reflections on these lead us to foundations in epistemology. Empirical statements must do justice to observations. True; but how is it that observations have this cognitive value? There must be objective control and some kind of communication to give them this cognitive value?

First things first. We pass from knowledge as an achievement, and what it implies, to truth and its tests, and from tests to their foundation. When we ask ourselves why we think we have knowledge about our world, we are likely to reply that our knowledge presents us with an amazing and detailed picture which has slowly been achieved and that it also enables us to predict and control. It follows that our tests have done their job. Taking perceiving as our paradigm, we note how we are guided by sensory data and how these are relevant to the application of our concepts. Here we have a sort of empirical if-then relation which is *a priori* in C. I. Lewis's sense and which, surely, throws light on the growth of the concepts. This situation does not seem to me to have been fully investigated as yet. However, I do think it to be the case that we can attain empirical certainty at the common-sense level. If so, according to my argument, our verified statements correspond to their objects. They could not give knowledge otherwise.

But we still have the "sixty-four thousand dollar" question. As Heisenberg points out in *Physics and Philosophy,* the most recondite theories play into observational data. The epistemologist must have some theory to account for the "cognitive value" of these. Since we do have knowledge about our world, and the statements mediating such knowledge are checked logically and observationally, the data must transmit to us something about their causes; they must be in responsible dependence to the objects we are helped to perceive. Here we have a causal theory of sensory data and not a causal theory of perceiving. That is, we are concerned with a study of the stimulus pole of perceiving, with the how, or mechanisms involved, and not with perceiving, as a referential claim built around response.

Terms such as "communication", "reproduction of pattern", "appearing", have arisen in this connection. Techniques of communication are being applied to the sense-organs. TV has some relevance to vision. It is clear that, through the sense-organs, the organism is being alerted to features of the environment. It is not a passive affair. An active supplementation of exploring and deciphering proceeds. Clues are often enough. Neurologists like Herrick speak of neural analytic and synthetic processes. Concepts arise as hypotheses to draw out as much as possible of information

The job of the philosopher, as I see it, is to help to give the proper perspective. So long as he did not have the correct view of perceiving as a terminative reference and characterization, he could not help to analyse the steps in the operation. He got confused and confused others.

To make a long story short, if sensory data are under the control of objects in such a way that these objects may be said to *appear* in them and if concepts are forged under such controls, the foundations of empirical knowledge are given a natural explanation. As I see it, the response of the organism helps to elicit information. Scientific technique has been added to biological technique. We interrogate nature.

We live in an age of electron microscopes and amplifiers. Even the Greeks were aware of the limitations of our sense-organs. Nevertheless, the point of departure must be a satisfactory theory of perceiving at the level of common sense. It is this that the critical realist has sought to work out.

I shall now try to sum up and emphasize perspective. I have tried to show that critical realism could account for truth. First of all, it did justice to perceiving as a directed operation terminating in reference and characterization. For its biological and psychological framework it took S-C-R as the functional unit. Thus it broke completely with the basis of traditional empiricism which had landed it in sensory subjectivism. Rather do sensations guide perceiving. Perceiving is objective in import since it is concerned with the things around us. This gives us a tidy linkage with ontology. Existence is not so much an attribute as a categorial recognition of the objective situation. That is why it is tautologous to say that this thing I perceive exists[1]. The new realism had good intentions but did not analyse perceiving carefully enough; and so fell back on a presentational *tour-de-force*. Critical realism is epistemologically dualistic only in the sense that it takes knowledge to be carried by statements. These must have a referential import which is prepared for by directed response which is lifted by language to designation. Knowledge, as an achievement, implies the correspondence, or adequacy, of the statement, a point recognized by the semantic theory of truth. However, correspondence is not a test; for no direct comparison between object and statement can be made. Verification is a complex operation involving logical and empirical tests. To complete its theory of truth and knowledge, epistemology must examine the reasons for the "cognitive value" of sensory data. This leads us back to biology and the role of the sense-organs. Theories of communication are here in order. But we must not forget that the organism is active and uses the sense-organs. The growth and role of concepts as factors in perceiving must be studied. In short, human perceiving is a very complex operation. The point I wish to make is that it is objective in import. That is why I call critical realism a form of realistic empiricism. Naïve realism reflects this import but has no idea of the nature and mechanism of knowing. How could it have?

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[1] Contrary to Leibnitz, I would say that the universe *just exists*, neither contingently nor necessarily These modals are not here relevant

All this represents a break with Cartesian dualism. Mind must be taken in a functional way. My double-knowledge approach seems to me the best bet. We can use both dispositional terms and occurrent ones. Because of a weakness in epistemology, Ryle seems to me to have oversimplified his revolt from Cartesian dualism. But it was a step in the right direction.

In conclusion I would like to make some comments on Wild and his neo-aristotelianism and on C. I. Lewis.

Wild is quite right in demanding a direct realism. It is the thing itself which we are perceiving. That is why knowledge has its ontological import. That is why categorial meanings are decipherments of the constitution of things. But it seems to me that Wild falls back on Aristotelian *forms* too readily. In accounting for the "cognitive value" of sensory data, I appealed to modern communication theory and the reproduction of pattern. But we must remember that the data must be taken up in a complex operation of guided response which flowers in reference and conceptual characterization. It is my belief that modern empiricism had to start with efficient causality, go through the fires of subjectivism, and then gradually work to a more adequate conception of perceiving. But I would like a *rapprochement* with the "Return to Reason" advocates. Concepts and reason play for me an important role, even in perception. Even in appraisal I am not an emotivist. But I do think that Wild and his group will have to face up to the mind-body problem more empirically.

As C. I. Lewis knows, our divergence lies in our theories of perception. He seemed to me to start from sensory presentations, pick concepts out of the air, deduce implications, and verify them. And he was persuaded that the critical realists were engaged in the fruitless quest of the copy-theory. It should be clear from the above analysis that I think of sensory factors as guiding a perceptual reference and characterization of direct, objective import. We do not first know ideas and then infer things which are like them. Now I welcome his appeal to concepts, which seem to be the instruments of knowing. But I should like to know more about their rise and testing. To me, the first stage of this seems to be in the perceptual situation itself. That Lewis starts from sensory presentations rather than from objective perceiving with its referential dimension seems to me to give it a phenomenalistic cast. But it is becoming apparent that Lewis is moving in the direction of realism, that he holds that meanings are applicable to denotables which are not sensory presentations. With this I heartily agree. It is my realistic empiricism. It may well be that we are nearer each other than seemed to be the case.

I do not think it is strange that an adequate base for theory of knowledge and truth has been hard to come by. The idealists built on subjectivism and were impatient of epistemology. As they saw correspondence, it was a self-defeating idea, as Blanshard still maintains. Only a Roycean God could directly test it. Coherence was, accordingly, the only alternative. I hope I have shown that testing works up to knowledge as an achievement, implying correspondence or adequacy. Perceiving is objective in reference and import but thought can work back to its framework and conditions. All of us realists felt that the pragmatists stressed verification and working, without doing justice to knowledge-claims. It was largely an attack on transcendental idealism. Thus an adequate theory of perceiving, which would do justice to both stimulus and response, remained a desideratum. By this reorientation I have sought to do away with the difficulty which, in the words of Bertrand Russell, "has troubled empiricism down to the present day"[2]. Sensations guide perceiving but are not its objects. Reference comes in at the response pole and is built up behaviourally and linguistically. Objectivity and interpersonal communication become operationally natural. Epistemology can thus link itself with ontology in a common-sense way. If I may judge from Quine, logicians may well welcome this kind of contact with "what there is". There is much analytic competence to-day. What it seems to need is a good starting-point.

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[2] See his "History of Modern Philosophy', p 612 f

NAÏVE REALISM AND ILLUSIONS OF REFRACTION

By M. ARTHADEVA

I

The facts of refraction provide the most serious difficulty for naive realism. A stick appears to bend when put into water or, equally, appears to bend when, from a state of being immersed in water, a part of it is thrust out of the water: the part of the stick in the water does not form a straight line with the part that is out of the water. Why does the stick look bent when it is not? This is the way the question has been formulated. How is it known that the stick is not actually bent? It has traditionally been assumed that it is the sense of touch that proves this, that gives evidence of the fact that water is not, by itself, a sufficient cause for actually bending sticks. It has been said, and is still maintained, that, if we run our hands down the stick, we would feel that it runs straight: if the stick were actually bent we would feel the bend with our fingers. I do not think the sense of touch can establish this as easily as is supposed. We often know that a thing is bent by feeling it, because we bend our hand or our arm to touch the thing when the corner has been turned, or because of the sharpness of the corner itself; and we have learnt by previous observations, based mainly on sight, that such sensations occur when we feel along bent things. Such knowledge, however, is not the result of direct apprehensions and is liable, as direct apprehensions are not, to be mistaken. If a man felt the edge of a table that was circular and completely smooth with the same part of his finger all the time (this can be ensured by his walking round the table while he was feeling it or by the table revolving) he would not know whether what he was feeling was straight or circular. A man who observed his fingers feel along the refracted stick would see his hand turn the corner with the stick: consequently he may, and sometimes people do, get the illusion that the stick itself feels bent.

How, then, do we know that the stick is not actually bent? We cannot know this for a certainty on the basis of feeling it with our finger in the way described; and here, as elsewhere, the sense of touch does not act as a privileged arbiter of the real. I am not suggesting that the sense of touch alone, without the aid of the other senses, cannot enable us to know when a stick is bent and when it is straight: it presumably can, if we take the discrimination that people totally blind from birth exercise; but how it does this is not easy to explain and does not concern us in this paper.

Even if the sense of touch is able to inform us that the stick is not bent, our perceptual problem would yet remain: we shall have to explain why it is that the stick looks bent and why our sense of sight is here providing us with wrong information. Philosophers, because of the phenomenon of refraction and because of what they have considered other visual illusions, have indeed given the sense of touch priority over the other senses: they have thought the sense of touch, unlike that of sight, to be directly in contact with the material world. This is to forget that the sense of touch also has its own "illusions", that there are instances when it too appears to be giving us wrong information about what actually exists. In any case, to say that the sense of touch is in direct contact with the real world, while the sense of sight is not, is to dissociate the object we touch and the object we see. We surely want to say that we touch and see the same object, that it is, for instance, the same table, which our hands touch, that we see before us. We do not want to deny that our sense of sight is in direct contact with the real world, that we *see things* as well as touch them. We are therefore forced to consider the problem of refraction afresh and provide an explanation, which is consistent with the assumptions we ordinarily make, of why the stick looks bent when partly immersed in water.

How do we know that the stick is not actually bent? If the stick is hollow, we can be aware, peering through it, that the stick has retained its straightness while it is partly immersed in water. Again, when we totally immerse the stick we see that it is straight and when we take it out of the water we again see it thus. We know, then, that the water does not actually bend the stick, does not exercise physical force upon it: we know that we have before us a stick which, for want of a better term, we shall say is only *visually bent*. As naive realists, we cannot say that the stick is actually bent in water, as Professor Ayer suggests naive realists should do. If we say this, we would have upon ourselves a whole host of difficulties: how are we to explain where the stick bends? We shall have to say that it is only the surface of the water that exercises force upon the stick. How are we to explain how the stick straightens itself where it was bent before and changes the points at which it bends on further immersion into water? To explain this we shall have to say that the water below the surface exercises a force in the opposite direction and unbends the stick. Why, again, does the angle at which the stick bends depend upon the angle at which it is immersed into the water? Why does the stick, when put in exactly at an angle of 90 degrees to the surface of the water, not bend at all? As we move the stick away from its vertical position, it begins to bend, and the angle of its bending increases. But on further increasing the angle away from the vertical, the stick appears to unbend itself, as it were, and, as the part of the stick above the water is level with the surface of the water, the stick is again straight. At this point, the stick is, of course, totally immersed in the water, though almost at a level with the water's surface. We have already noted that when the stick is totally immersed in the water it is completely straight. How are we to explain the erratic "behaviour" of the stick? To say that the stick actually goes through all these changes of shape will get us into insoluble difficulties. We have a meaning for the words "changes of shape" applied to situations when, on the exertion of physical force, a straight bar, for instance, is bent: for the bar to regain its earlier shape or to be altered in a new way, physical force must be applied. To say that the water bends and unbends the stick, depending on the angle at which it is immersed, and sometimes does not exercise any force on it at all, is to attribute strange and unaccountable causal powers to the water. The water, moreover, bends things without exercising a force that we can feel in any way: when we put an arm into the water we do not feel any physical force upon it. Yet it too looks bent like any other physical thing. We cannot then say that the stick really changes its shape when immersed in water. We all know that still water does not exert physical force in this way. Therefore, if we wish to be consistent in the language we use, we cannot employ the phrase "changes of shape" in describing what happens when a stick is immersed in water.

What shall we do? Some philosophers have concluded from this and similar phenomena that immediate perceiving does not, on all occasions, give us the real qualities of things; they have been led to make a distinction between veridical and delusive perceptions. Once we do this, however, it is clear that we cannot remain naive realists. According to naïve realism, as I interpret it, to say of a thing that it is perceived, i.e. seen or touched or heard or smelt or tasted, implies saying that it actually exists and that it actually possesses the character that it is perceived to possess. No distinction is made in naïve realism between normal or veridical and abnormal or delusive perceptions: all perceptions, according to naïve realism, are veridical. Since we cannot, as Ayer suggests, say that the stick actually bends, what can we say? We can, of course, give up naive realism and adopt the terminology of sense-data, but this is not what we intend to do.

Considering the large number of philosophers who have dealt with the problem of refraction, it is a remarkable fact that none of them have noted that the stick in water does not in many respects look like an ordinary bent stick. In fact, in its *essential* characteristics the partly immersed stick does not resemble a bent stick. This, strangely, is what philosophers have not mentioned at all. Professor Austin has said in lectures[1] that a stick partly immersed in water does not look exactly like a bent stick out of the water. He wittily pointed to the fact that we could not help seeing the water. All this is true and needed saying: we are not deceived by the phenomena of refraction, for they occur in conditions which can be distinguished by us as conditions for refraction. When water, hot air, the use of magnifying or diminishing lenses cause objects to look to us differently from the way they usually look, we are not left without means of finding out that we are seeing in other than usual conditions, that we are in fact seeing things in water or in hot air or through extraneous lenses. Notwithstanding this, our main problem still remains unsolved. The final question is not whether we are or are not deceived by refraction but whether we are entitled, because of the facts of refraction, to use words like "bent" and "straight", and other words referring to shape, of material things: this is how the facts of refraction become philosophically disturbing.

When we say of a thing that it has a certain shape, that it is straight for instance, we are, according to ordinary assumptions, describing the thing by one of its real characteristics or qualities. When the same thing, without undergoing what we call a real or physical change, is seen under different conditions as bent, the question arises as to whether the terms "bent" and "straight" properly apply to the thing at all. We are not willing

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[1] I refer to the lectures entitled "Sense and Sensibilia", first given at Oxford in 1946 and repeated in Michaelmas Term 1955 Though these lectures are no doubt famous, it is greatly to be regretted that they have not been published Professor Austin, in my opinion, has shown more decisively than any other philosopher that the sense-data theory of perception has no logical ground whatever: the "illusions" we are faced with in perception do not warrant the sense-data theory, nor does it even remotely answer the perceptual problems actually raised by the facts of illusion, so called.

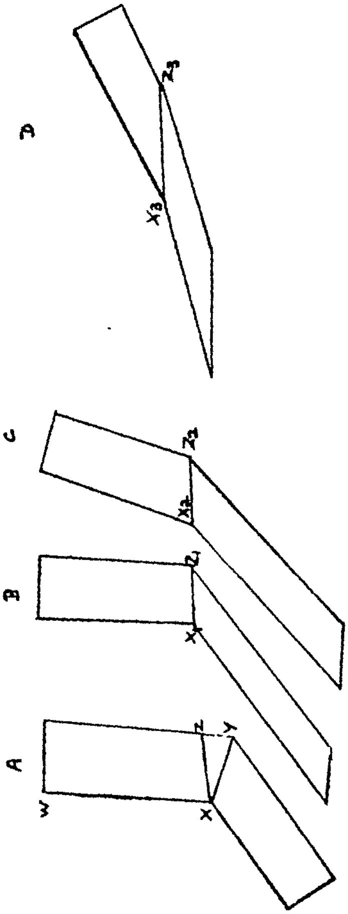

Fig. 1

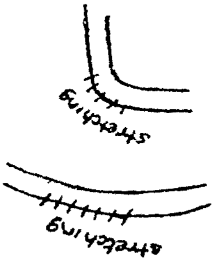

Fig. 2

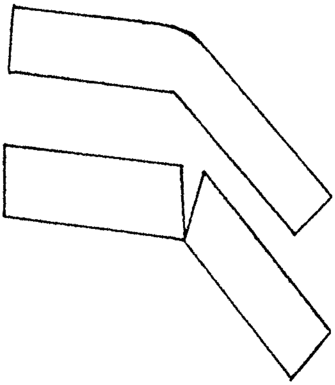

Fig. 3

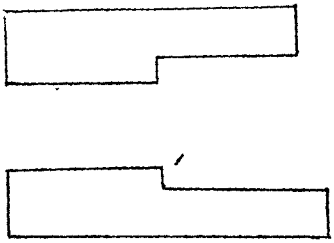

Fig. 4

to say of water or any liquid that it is, for example, spherical or box-shaped, because liquids have no shape themselves but take on the shape of the vessel that contains them. But of solid things, especially those that are rigid, we do want to say that they have definite shapes, that they are, for instance, straight or bent, round or square. The facts of refraction, and indeed of perspectival distortion, create difficulties in the way of our attributing shapes, simply and without qualification, to material things. That we do use words like "bent" and "straight" as simple attributes in ordinary and scientific discourse there is no doubt. The question for philosophy to decide is whether we are justified in so using them.

The stick in water, as we said in a previous paragraph, does not resemble in many respects a bent stick. We add that in their *essential* characteristics refracted sticks and bent sticks are visually different. In Figure 1, A is a drawing of a bent stick and B, C and D are drawings of refracted sticks. It is not easy to bend a stick at two precise points: there is usually a stretching at the outer side of the bend (vide Figure 2). We can avoid, however, the stretching on the outer side if we compress the inner side of the bend: we have thus, with difficulty, made our stick A bend at two precise points to make it resemble as closely as possible a refracted stick. Yet the differences between bending and refraction remain. If the stick A is bent at a point X on one side, this involves an equal bending of the stick at a point Y on the side behind: the angle WXY must form 90 degrees. The stick cannot be bent at the points X and Z, which when joined form with the side of the stick an angle less than 90 degrees: an attempt to do this will crack or stretch the back of the stick (vide Figure 3); in neither case will the stick be said to *bend at the points X and Z*. The refraction of the stick occurs, however, at the points X and Z, points on a level with the surface of the water: the angle formed by this horizontal line and the side of the stick is always less than 90 degrees.

Imagine a stick bending at the points at which the stick D (Figure 1) is refracted! It is impossible for a stick to bend at points X_8 and Z_8. As the stick nears the vertical position on immersion the angle WXZ gets nearer 90 degrees and the refraction that takes place resembles bending more closely. But no instance of refraction does or can take place at points which when joined form right angles with the sides of the stick. We must not forget that a stick is refracted only when it is immersed at an angle with the water's surface. If it is immersed at right angles to the surface of the water, the immersed portion of the stick does not appear to bend. The stick is always refracted therefore at points which when joined form with the side of the stick an angle of less than 90 degrees. A stick cannot ever bend *exactly* at two such points. There is a logic or necessity in bending to which all bending must conform. If we attempted to bend a stick at the points at which refraction takes place, the back of it, as we have illustrated in Figure 3, would crack, not bend, or would stretch. In any case, the stick will not bend neatly at the points at which it is refracted and it is the preciseness with which refraction takes place at these points that distinguishes it from bending.

We can also point to the fact that the refracted part of the stick is always narrower than the part above the water. That the refracted part of the stick is narrower than the part unrefracted shows us that what we see before us is not a bent stick. If it is a bent stick that we see, let us try to imagine how it would look if it were unbent or straightened. Figure 4 is an illustration of how the stick under discussion would look if it were straightened. We could see from this that what we have before us is not a straight stick bent. The important point is that refraction and bending are visually different, though they are also visually alike in some degree and to such a degree that they can be confused.

Refracted sticks, therefore, turn out on closer examination to be essentially different from bent sticks. Refracted sticks, in the last analysis, ***do not even look bent.*** If sticks were refracted at points where they could bend and there were no visible difference between refraction and bending, as philosophers who hitherto discussed this question have assumed, then our problem could not have been treated in this way.

II

Our problem is only partly solved, it may be rightly said, for a fresh philosophical difficulty arises. Though we may not ask, "Why does a straight stick look bent when put into water?" because, strictly speaking, it does not look bent, cannot we inquire, "Why does a straight stick not look straight when put into water?" Though a refracted stick is not a bent stick and does not look like one in all its details, it also does not look in any case like a straight stick. Why does a stick, then, when partly immersed in water, look otherwise than it actually is?

The stick is actually straight: why does it not continue, therefore, to look straight when half immersed in water? If perceiving always gives us characteristics which are real or actual, why does the stick not continue to look straight even after it is thrust into water? It must be admitted that the stick partly immersed in water does not look as it does out of the water, or as it does when totally immersed. How are we to explain this change of appearance?

The problem is indeed a difficult one and we must be careful at the outset to understand exactly what it is. There are other problems connected with apparent changes in the shapes of things which we have to be careful to distinguish from the apparent change of shape effected by refraction. When we look at a thing from different angles, for example, it appears to us to have different shapes: as we all know, a penny looks elliptical and not round when viewed obliquely. We have to be particularly careful when we deal with the problem of refraction because there appears to be some confusion among philosophers about the very facts of refraction[2]. It has been said that the stick appears bent when viewed from certain angles and that viewed from other angles it appears less bent or even straight. This is contrary to fact: the stick looks bent from whatever angle you look at it: that is, as long as you look at it through the surface of the water and not through any of the sides of the tank in which the water in contained. It is true that from some points of view it is less easy to see that the stick is bent than from others: if you look at the stick from the direction towards which the stick bends, you will hardly see the bend. The same is true, however, even when we look at actually bent things. If we bend a match, for instance, and look at it from the direction towards which it bends, it is not easy to distinguish that it is bent. It is always easiest to see the bend of something if one looks at it from a side. No one intends to say that the match is not really bent, because we do not distinguish its bend from a certain point of view, or that its degree of bending depends upon the point of view from which it is seen. We are willing to concede that why the actually bent match and the apparently bent stick do not appear bent, when we look at them from a certain point of view, is a perceptual problem: but it must be clear that it is a different problem from the one we are discussing here. In refraction we are concerned with why a stick which is actually straight looks bent from any point of view at all: we are not concerned, for the moment, with the different perceptual problem of why its apparent bend is better visible from certain angles than from others. The difficulty about refraction is precisely that the partly immersed stick is exactly like the actually bent match in this respect: its apparent bentness is as much a public fact as the actual bentness of the match and is independent of the point of view of the observer, though, of course, it is not independent of the angle at which the stick is immersed in the water. As everyone knows, the stick does not look bent when immersed vertically in the water.

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[2] Vide Toulmin: *The Philosophy of Science* p 61: here it is stated that the apparent position of the stick depends upon the angle from which it is viewed

How are we to explain why the stick looks bent? Why do the two parts of the stick not form a straight line? It must be made clear at the outset that we cannot solve our problem by making a distinction between veridical and delusive perceptions and by characterizing one of them, namely, seeing the stick partly immersed, as delusive. This is what philosophy, faced by the facts of refraction and what it has considered other visual illusions, has hitherto done. Not being able to find a logical justification for the distinction between veridical and delusive perceptions, philosophers have gone on to deny that we are ever in contact with the real world: they have supposed that we sense sense-data or have ideas when we perceive and they have raised for themselves one after another the familiar solipsistic problems. By doing this, philosophy has in fact avoided its real tasks and created for itself fictitious difficulties. By throwing doubt upon the thesis that our senses are in direct contact with the real or material world, it has rejected our only available means of knowledge of this world. The facts of refraction are equally part of our knowledge of this world: it is these facts which show us that media have different optical properties; perceptions of sticks partly immersed in water are as veridical, therefore, as perceptions of sticks wholly out of the water. That things, when they are partly immersed in water, look different from the way they look otherwise does not show us that some of our perceptions are delusive.

What, then, do the facts of refraction prove? Do they show us that the term "straight" cannot be applied in a simple and unqualified way to the stick? The stick is straight when seen under one set of conditions and not straight when seen under another: therefore, straightness or a real shape cannot be attributed to the stick independent of the context in which it is seen. Because the shapes of things look different when the things are seen in different conditions, are we to go on to maintain, as Professor Austin did in his lectures, that things cannot be said, in a simple and unqualified sense, to have real shapes?

Though Professor Austin did wish to say of the stick in water that it was straight, his criticism of the term "real shape", as it applied to things seen through microscopes and telescopes, implies that shape-words in general could not be predicated simply of things. "Straight", "bent", "round" and "square" are more basic terms, it is true, than "horse-shaped" and "cat-shaped", which are the terms Austin dealt with, but they are equally terms of shape. If "horse-shaped" and "cat-shaped" cannot be predicated in a simple and unqualified way of things, then the more basic shape-words also cannot be used in this way.

This is then one possible direction in which we may proceed in order to try to solve our problem of refraction: we can say, not that the perception of the stick in water is delusive, but that the word "straight" cannot be applied to the stick when seen in this condition. To the question, "What can be said of the shape of the stick in this condition?", we can answer that it can be said to be "bent" or be "bent-like". Our problem of refraction, according to this argument, is caused by the fact that we use terms like "straight" and "bent" of the stick, as though these terms applied to the stick irrespective of the conditions under which it is seen. Once we give up speaking as though shapes were intrinsic qualities of things we shall solve or rather dissolve our problem of refraction.

It appears to me that this answer has many serious disadvantages. Besides rejecting as invalid the way shape-words are used in ordinary language, it takes no account of the fact that the stick partly immersed in water is not bent and can be shown not to be bent. If the stick in water really looked like a bent stick, then there would be greater need to be dubious about the simple application of our shape-words. Since the stick in water, on closer examination, does not look bent, it does not seem necessary to go as far as rejecting ordinary usage. As was pointed out before, we do not use words like "straight", "bent", "round" or "square" of liquids, because liquids change their shapes depending on whether they are seen in straight, bent, round or square containers. We similarly do not use shape-words of air or of other gases. But solid things like sticks, rocks, trees, have definite shapes. The shapes of these things do indeed change: sticks break or get bent, rocks change their shapes when they decompose and trees when they grow. These changes are explained, however, by physical or natural processes:

what these physical or natural processes effect are changes of shape; the sticks, rocks and trees change from one definite shape into another.

When a stick is seen partly immersed in water it does not, as we have amply pointed out, change its shape. What is it then that changes? How are we to describe what has happened? If we simply say that the word "straight" cannot be used of the stick, we are not making any use of the fact that we are now seeing the stick *in different conditions*: that we are now seeing the stick partly immersed in water where before we saw it out of the water. We can see the water, as was pointed out before, and see that the stick is partly immersed in it. If in order to solve our problem we simply say, as recommended, that the stick in itself is not straight, we are ignoring any significance our seeing in altered conditions may have. If we sometimes saw a stick straight and at other times saw it bent without any change occurring in our conditions of observation, then, it is agreed, we would be reduced to saying that shape-words cannot be predicated of the stick. It may be argued, however, that what Professor Austin is maintaining is not that shape-words cannot be attributed to things but that they cannot be attributed without qualification: according to him, we can predicate "straight" of the stick if we add the qualification that the whole of it is seen in air: we cannot predicate "straight" without such a qualification.

The question we wish, however, to raise is, "What sort of qualification is this?". What do we mean when we say that the stick is straight *if it is seen* out of water, in air? We can say, of a rubber ball, that it is round if it is not being trod upon; or, of a tree's branch, that it is straight when the wind does not press on it. When we qualify our predication here, we do not imply that the ball is not round *in itself* or that the branch is not *really* straight. In the case of the partly immersed stick, on the other hand, when we say of it, "It is straight if it is seen out of the water", we are recommended to imply that it is not straight in itself, not really straight. Our qualification in the case of the ball and the branch does not, again, preclude us from saying that the ball is really flattened when stood upon and that the branch is really bent by the wind. In the case of the stick, however, when we say it is seen bent when partly in water, we cannot conclude, as we did in the case of the branch, that it is really bent The kind of qualification made in the case of the stick is thus of a radically different kind.

To solve our problem by qualifying the words "straight" and "bent" in the way suggested leads us into difficulties: the qualification made does not conform to the usual kind of predicate-qualification that we find in the language. What is more, the qualification contradicts the assumption that what is perceived is independent of the perceiver and the conditions under which the perception takes place: this assumption is contained in the naive realist view of perception and, as I hope to show, it colours ordinary-language statements about perceiving and its objects.

We feel the need for an explanation when a thing looks otherwise than it usually does. Suppose someone were to say of a thing, "Its shape is now seen to be different", do we not always inquire, "Why is its shape seen to be different? What explains this?" Why do we expect an explanation? Our need for an explanation is satisfied if we are told that the thing itself has changed or has been altered. Why are we satisfied by this explanation? We are satisfied by this explanation because it gives us a reason why the thing looks different: it looks different, says the explanation, because it has changed, because now it *is* different. The explanation satisfies because it preserves the assumption that a thing looks as it is, that when we perceive a thing we are aware of its real characteristics.

This assumption is fundamental to naïve realism: according to this perceptual theory, whenever we see, touch, or otherwise perceive, we are aware of a material thing and its characteristics. Now, by definition, a material thing is independent of the perceiver, for the same material thing is perceived by more than one perceiver: it is an object which furthermore exists even while it is not being perceived. This material thing does indeed change when other material things or forces affect it, but our perception of it does not in any way alter it. That is why, if we see it differently, we do not say that it has changed but that it *appears* to have changed. The word "appears" and its equivalents, "looks" and "seems", when used in this context, signify that the object has not really changed: they imply in fact that the object is the same or retains the same characteristics even though it is seen as being different. To say, therefore, that the characteristics of objects, their shape, for instance, cannot be defined apart from the context in which these characteristics are perceived, is to ignore the purpose for which the words "looks", "seems" and "appears" are employed in these contexts. Furthermore, if the shape of a thing is not independent of the conditions under which it is perceived, can we consider the thing to be so independent? If we consider that the thing is independent of the conditions of perception while its shape is not, then we have fatally separated the thing from its characteristics, a separation which we shall find hard to justify. If the thing itself is equally dependent, like its shape, upon the conditions of perception, then the object we perceive is not a material thing by definition and we are not really naive realists: we are nothing but sense-data philosophers in disguise.

We cannot give up the thesis that the characteristics or qualities of a thing remain unaffected by the conditions of perception without adopting sense-data positions or getting involved in Lockean-like distinctions between things and their characteristics. However hard it is to solve the problems raised by the so-called illusions, we must nevertheless try to deal with them without abandoning the naive realist point of view: to retreat from its firm and clear doctrines is to throw oneself back into the scepticism from which modern philosophy is trying hard to escape.

The stick in water, then, is not bent: neither the stick nor its shape has changed. We have too many advantages to forgo if we abandon this view. The stick out of the water is seen straight: totally immersed in water it is again seen straight; in other media or through lenses, if the whole of it is seen in one medium, the stick is again seen straight. It is only when the stick is partly seen in one medium and partly in another that it is seen bent. Even in these conditions it is not always seen bent: if, from one medium, it is vertically immersed into another, it is still seen straight. It is only when the stick is immersed at an angle with the surface of the water that it appears bent: this is the only trifling condition in which it appears other than it really is. We have surely too much to our advantage to need to abandon naive realism or adopt the defeatist position that the shapes of things cannot be defined apart from the context in which they are seen. We have already pointed out that we can also perceive the conditions under which we are seeing: we can see that part of the stick is in water and that part of it is above the water and even that the part above the water is at an oblique angle with the surface of the water; we can thus see every detail of the disturbing conditions which make the stick appear otherwise than it actually is.

All this, it may be said, only points to the fact that refractions are in no sense to be called illusions: they are as veridical as perceptions of sticks in normal conditions. We have not yet solved the problem, however, of whether, because of the phenomena of refraction, we can call the stick in itself straight or bent: this is our major difficulty. The answer that suggests itself on first thoughts is that we should re-define straightness in such a way as to apply to the refracted stick. We have amply shown, after all, that the stick, strictly speaking, it not bent, because bending never takes place at the points at which refraction occurs. Why can we not therefore define straightness in such a way as to apply both to the stick as it is seen out of the water and to the stick as it is seen partly in the water, define it in terms of the common characteristics which straight and refracted sticks share? We could then say simply that the partly immersed stick looks straight, according to our new definition of straightness.

This does not seem to be a satisfactory way of solving our problem. If our refracted stick, on closer examination, does not exactly resemble a bent stick, there is no difference between it, as it looks, and a *crooked* stick of exactly that shape. If our definition of straightness covered the kinds of shape that refracted sticks take on, we should have to include many actually crooked sticks in the same category. To define straightness to include crooked sticks of certain kinds would be to travesty the notion of straight: it would furthermore make the term "straight" rather technical and complicated and hardly suitable for use in ordinary language.

We cannot then re-define straightness and hope to solve our present perceptual problem. The position, or the place in which a thing is, is a relative characteristic, something that is defined in terms of *other* things; but the shape of a thing is an absolute characteristic, something that is defined in terms of the thing alone. Yet the notions of position and shape are related: the shape of a thing is determined by the spatial relationships of the parts of that thing to each other. Though the relationships concerned are internal ones and do not involve other things, yet the relationships are spatial. It would be curious if apparent changes of spatial position caused more than apparent changes of shape. We must be careful then not to admit that a real change of shape has occurred. But how shall we avoid admitting this? We seem to have tried all possible ways out and none of them has succeeded.

Can we not solve our problem by making a distinction between the seeing of a thing and the seeing of its shape? The stick itself is seen: actual parts of the stick are seen above water and other actual parts of it are seen in the water; we can say without a doubt that we are seeing the stick because we are seeing its parts. To see the shape of something, however, it is not sufficient to see its parts: we must see the parts of the thing *together.* There are many occasions on which we can be said to have seen a thing but not to have seen its shape. Suppose something were shown to us in parts, each part uncovered at a time but never two parts together, until all the parts of the object are seen. We can then truly say that we have seen the whole object: yet it is also true that we have not seen its shape. We can again be said to see the whole exterior of a huge building if we walk round it and yet not be said to see its shape. To see the shape of something all parts of its surface must be seen in a unitary from or *Gestalt.*

On some occasions, then, like the ones we have discussed above, we can be said to see a thing and not see its shape. We must not forget, however, that when we see the parts of a thing which are uncovered to us one at a time, we do see *the shapes* of *these parts,* and that, while we walk round a building, we do see the shapes of its different portions. If we do not see the shape of the whole thing, it is because under these conditions we do not see the thing as a whole: we do not see all the parts of the thing together or at the same time. The parts of our partly immersed stick, however, are seen at the same time. We seem thus to be left with our problem yet unsolved.

III

When we are faced with a perceptual problem (i.e., when we find that a certain characteristic of a thing appears different in different conditions of perception), there are two main ways in which we can attempt to solve it. We can say either that the characteristic of the thing in question is a relative characteristic or that our perception of it under certain conditions is faulty: we can qualify the characteristic or, keeping that constant, qualify the perceiving. With regard to the problem of the shape of things, we have tried up to now to qualify this characteristic: we have tried to say that the term "shape" is relative and is to be defined in terms of the conditions under which it is perceived. This solution has not been satisfactory because it has run counter to our fundamental assumptions. Let us now try the other of the possible ways of solving perceptual problems: let us try to find some means of saying that in certain conditions of perception our perceiving of shape is in some way faulty.

Before we attempt to solve our problem in this way, let us ask ourselves again what exactly our difficulty is. The question that puzzles us is why the stick looks bent when partly immersed in water at an angle. Certainly, the part of the stick in water does not look bent and the part above the water also retains its straightness: both parts, taken separately, are straight; but taken together they are not. We cannot say that in these conditions the whole stick looks straight: the whole stick simply does not look straight. The question we may raise, however, is whether, because they are seen in different media, both parts *should* be taken together. We must question whether our seeing a thing partly in one medium and partly in another is a proper way *of seeing its shape.* Since we cannot say that the stick looks straight, that it looks its real shape under these conditions, we must solve our perceptual problem by throwing doubt upon the validity of our seeing the stick's shape in these conditions. We can say that we cannot in these conditions see the shape of *the whole stick,* though we can see the shapes of its two parts, the part above the water and the part below it.

We must clearly point out, in case it be misunderstood, that we have not characterized our perception under these conditions as delusive: we do see what is actually there; we do see actual parts of the actual stick. And we do see the *real shapes* of these parts, and the parts are really straight and are seen to be straight. But in these conditions we do not see the shape of the stick as a whole. Our perception of the whole stick is faulty because both parts of the stick are seen separately: though both parts are seen *at the same time,* they are not seen *together,* in a way we shall soon specify. We do not see the shapes of the whole stick, therefore, because we do not see the stick as a whole: we see it in parts; each part is seen separately or through a separate act or kind of seeing. Our seeings occur at the same time but are not on that account to be taken as one seeing.

In an earlier paragraph, we cited occasions when we could be said to see a thing but not to see its shape. We see the whole of a building when we walk round it, but we do not see its shape as a whole because our seeings of the parts do not occur together or at the same time. The shape of a three-dimensional object cannot, strictly speaking, be said to be seen by us because we cannot see all parts of the object at the same time: we cannot include within one act of seeing the whole object. On some occasions, however, all or most of the different parts of a thing can be seen by us at the same time: this happens if we look at a three-dimensional object with a mirror behind it; we can now see most or perhaps all parts of the object *at the same time.* Yet we would not say we see the object *as a whole,* as the one object it actually is, because the part of it seen in the mirror is visually disconnected from the rest. Though the seeings of the parts occur at the same time, we do not on that account consider these seeings as one seeing: they are deservedly to be called separate seeings or kinds of seeing.

We can, even at one moment, have two seeings. In a previous article[8] we said that, when we see double, we see one thing twice. Here again, at one moment in time we have two seeings. Though talk about seeings or acts of seeing is not common in our language, we are forced to introduce it in order to avoid saying, in cases of double vision, that we are seeing *two things.* We are not seeing two things when we see double, but are seeing one thing twice. From saying this the transition is easy to the talk about seeing in the plural, to acts of seeing.

Ordinary language has developed in order to deal with the situations in life we find usually and commonly. Its equipment is not sufficient to deal in a precise way with extraordinary situations. Science is necessary precisely because of this. Science does not, or at any rate should not, invalidate the distinctions made by ordinary language: it should sharpen these distinctions and make new ones which are consistent with the old. Cases of refraction, reflection and double vision are comparatively unusual and we are content in our ordinary language not to make special allowance for them. To describe our seeing of ourselves, for instance, in mirrors, we indifferently use in ordinary language one of the following expressions: we say, "I see my reflection" or "I see myself by means of a mirror, through a mirror" or simply "I see myself in a mirror". These expressions are by no means equivalent to one another, though they are used indifferently of the same situation; nor are some of the words in them used as they ought to be used. When I say, "I see myself *in* the mirror", I do not use the word "in" in the same way as I do when I say, "I see the chair *in* the room". Again, "To see my reflection" and "to see myself" are not equivalent in meaning if we consider, not the use to which the phrases are put, but their logical form alone: the meaning of the word "my" in "my reflection" is not the same as when, for instance, we use it in "my body" or "my arm".

To deal with these unusual cases of vision we need, then, to sharpen the distinctions we already have in ordinary language and to make new ones. The science we have been used to since Newton has employed the term "mirror image" of what we see in mirrors and has distinguished between *things* and that which we see in mirrors. It tells us that what we see in water is not the real stick, for instance, but an image of it. These distinctions, as we have shown, run counter to the logic of ordinary language. In ordinary language what we see, when we do, is a thing which is in the world and is where it is seen to be. The image in the water and in the mirror are not supposed by the scientist to be things and are, therefore, not where they are seen to be: in fact, they are "in the mind" and not in the world. We may well question, therefore, whether we *see* them, whether the word "see' is used here in the way we use it in ordinary language. We need a science of seeing which does not contradict the assumptions and distinctions which we usually make: it must deal with the extraordinary cases of vision by sharpening and refining the terms of ordinary language and not by distorting them.

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[8] "Naive Realism and Illusions of Reflection", this *Journal,* Dec 1957, pp 155-169.

We must not forget that when we see the stick in water we see *through* the water: because part of the stick is actually in the water we have to see through the water if our vision is to reach it. Similarly, though we do not bother to remind ourselves of it, we see the part of the stick above the water through the air. In fact, the further away we are from the stick, the more air we see through when we see it. Because air is usually completely transparent we tend to neglect its presence, but we must not forget that it exists and is as material as other things. Seeing through air, seeing through water, likewise seeing through other media or through lenses, are different kinds of seeing. In ordinary language we have not learned to qualify seeing. We say we see things *in water,* without taking special notice of the fact that, because the thing is in water, we see *through water.* We qualify the thing, its position, when we say that it is in water, but we do not qualify our way of seeing it, by adding that we see through water.

In ordinary language we neglect to mention the different kinds of seeing for the further reason that the things themselves do not always look altered when we see them differently. The shape of the stick totally immersed in water does not look different from its shape in air. A tree preserves the same shape in general when looked at from a distance or from close quarters: things seen by the naked eye or through telescopes are roughly similar in shape. In ordinary language we are content if we can approximately describe reality.

To make our ordinary language equipment more precise and capable of dealing with the problem of illusion, we must learn to qualify seeing. When we see a thing in water we see it differently: the thing is not now different but we now *differently see.* The word "differently" adverbially qualifies the seeing and not the thing seen. We had first tried, in dealing with the illusions of perception, to qualify the thing: and, finding that we could not (because things do not change unless they are physically affected), we went against our common-sense assumptions and posited images or sense-data as the objects we see, in an attempt to solve our problem. We did not, for some reason, have the courage to qualify our seeing and distinguish it into different kinds.

When we see through water, through air, through different media or through different lenses, we see differently, see in different manners or ways, have different kinds of seeing. These different kinds of seeing may occur together or at the same time, but do not because of this become the same seeing or the same kind of seeing: when we see a stick partly immersed in water, i.e. when we see part of it through air and part of it through water, we are differently seeing each part of it. We do not apprehend its shape when we see it thus because we do not unitarily see the whole of the stick: each part is differently seen, apprehended through a different kind of seeing. Seeing by means of a mirror is likewise a different kind of seeing from seeing a thing directly. When we come to cases of double vision, we are faced not with different kinds of seeing occurring together but simply with different seeings occurring together: two seeings of one kind occur at the same time.

Talk about seeings, about different kinds of seeing, no doubt sounds unfamiliar: in fact these ways of speaking are unfamiliar, these distinctions not being found, as we have noted, in ordinary speech. Some distinctions such as these, however, must be made in philosophy if we are to solve the problems raised by the so-called illusions of perception[4]. On the basis of these distinctions we can explain why the stick partly immersed in water does not look its real shape. The stick does not look its real shape when seen partly in air and partly in water, not because it has changed or its shape has changed: nor yet because in the water we do not see the real portion of the stick but an image a few inches above it. The stick does not look its real shape because we differently see the two parts of the stick: our seeing of one part is of a different kind from our seeing of the other.

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[4] The case for distinguishing seeing into different kinds is, I believe, given support in Professor Austin's account of perception He states, in his lectures referred to previously, that the phrase "indirectly seeing" as opposed to "directly seeing" has application: he refers to seeing in mirrors as an instance of "indirectly seeing".

We do not commit ourselves in this account to saying that the shapes of things are dependent upon the conditions of perception, that they cannot be defined independently of the medium in which they are seen Our stick is straight and its straightness is independent of the medium in which it is seen. In air it is seen straight and in water it is again seen straight. When placed in other media too it does not appear other than straight. We can rightly say, therefore, that the term "straight" can be applied to the stick independent of media, independent of the conditions of perception. If it is not seen straight when part of it is seen in air and part in water, this is not because the term "straight" does not apply to things independent of media: it is because, in these conditions of perceiving, we do not see the two parts of the stick *together,* in the sense not of seeing them at the same time but of seeing them by means of one and the same seeing.

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THE ONTOLOGICAL PROOF REVISITED

By Nicholas Rescher

> "I began to ask myself whether
> there might be found a single
> argument which would require no
> other for its proof than itself
> alone, and would suffice to
> demonstrate that God truly exists."
> —St. Anselm

I. Preface

For years now it has been the almost universal practice of philosophers to dismiss Anselm's ontological proof of the existence of God. Perhaps the most recent significant exception, surprisingly enough, is the brief acceptance of this argument by the young Bertrand Russell. In his essay on "My Mental Development", Russell records:

> "I remember the precise moment, one day in 1894, as I was walking along Trinity Lane [in Cambridge], when I saw in a flash (or thought I saw) that the ontological argument is valid. I had gone out to buy a tin of tobacco; on my way back, I suddenly threw it up in the air, and exclaimed as I caught it: 'Great Scott, the ontological argument is sound'."[1]

This quaint episode apparently constitutes a high-water mark for the ontological proof in recent philosophical history. It is now quite thoroughly and completely out of favour, among philosophers of religious and anti-religious persuasions alike.

The major apparent reason for this general rejection of the ontological proof is a number of seemingly decisive and insuperable objections which have been directed against its logical validity in the first instance, and its theological serviceability (even were it to be valid) in the second. These objections have now beeen advanced so repeatedly and so persuasively as to have become classical. Their aggregate effect has proved quite fatal to the ontological proof. The purpose of this paper is to show that it is possible to reformulate the ontological argument in such a way that all of these traditional objections to it lose their force, so as to render the argument once again worthy of serious philosophical consideration.

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[1] *The Philosophy of Bertrand Russell*, edited by P A. Schilpp (Evanston and Chicago, 1944), pp. 3-20, quoted from p. 10. "I shall venture to add an observation, that the argument a priori [for God's existence] has seldom been found very convincing except to people . who have accustomed themselves to abstract reasoning, and who finding from mathematics, that the understanding frequently leads to truth, through obscurity, and contrary to first appearances, have transferred the same habit of thinking to subjects where it ought not to have place." Hume, *Dialogues Concerning Natural Religion*, part IX.

II The Ontological Proof and Some Objections

It will be convenient for my purpose to consider the ontological proof in the format given to it in Descartes' Fifth Meditation:

> "I clearly see that existence can no more be separated from the essence of God than having its three angles equal to two right angles can be separated from the essence of a triangle And so there is no less absurdity to our conceiving a God (that is, a Being supremely perfect) to whom existence is lacking (that is, to whom a certain perfection is lacking) than to conceive a mountain that has no valley."[2]

In this Cartesian version, the ontological proof has two steps (1) it is posited that the nature of the concept of God (i.e. the definition of the term) is such as to embrace existence within its scope, and (2) it is contended that, as a consequence, to deny the existence of God is to make a statement that is logically self-contradictory Thus on Descartes' formulation, the ontological proof concludes God's existence *deductively* from the definition of the term

From the very first, serious objections have been advanced against this argument. Three criticisms in particular attained to the status of classical objections, and these seem to continue to carry the main burden of justifying the widespread current rejection of the ontological argument.

The first objection turns on the logic of the definition of the term "God" that is basic to the ontological proof. The argument rests upon an explicit definition of this term *per genus et differentiam* which is such as to include existence within the set of attributes that constitute the defining, essential attributes The logical admissibility of such a procedure depends upon the thesis that *existence* has the status of a predicate, i.e., an attribute-denoting term. But, as logicians from the time of Hume and Kant have consistently and convincingly argued, existence must not be classified as an attribute.[3] This being so, neither can it be an essential attribute, and so cannot qualify as an item to be included in the defining content (i.e., intension) of a term.

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[2] *Meditations on First Philosophy,* Fifth Meditation, translation adapted from that of R M Eaton in *Descartes Selections* (N Y., 1927)

The second objection protests against the logic of the procedure of "defining into existence". The point and objective that is at issue in a purported ontological proof is, after all, God's existence. To derive this desideratum from the definition of the term is, in effect, to settle the matter by *fiat*. For to insist upon defining a term in such a way as to preëmpt a question regarding it that is at issue, and so to claim the desired consequence as a deductive inference from the definition, is to engage in what is of necessity a circular procedure. The nature of valid deductive reasoning is such that we cannot by means of it extend the limits of our facts, however useful it may be in extending our explicit articulation of the information concerning them: the information expressed in the conclusion must always be also asserted, however covertly, in the premises. If, therefore, an existential conclusion is drawn deductively from premises, these must be such as themselves to presume or presuppose the existential fact in question, so that we cannot actually advance to the establishment of any existential fact other than one already covertly assumed or decreed.

The foregoing two objections to the ontological proof have been derived from logical grounds. The third traditional objection derives from theological considerations. The conception of God upon which the ontological argument proceeds is by no means satisfactory from a religious standpoint. The "God" of which it speaks is a logical amalgam adjusted to the logical convenience of an existence proof, and this makeshift is not recognizably to be identified with the deity worshipped in any operative monotheistic religion. The ontological argument finds God "at the end of a syllogism" and not "in the hearts and souls of men".[4] Thus, even if this proof were logically sound, its theological serviceability would be insignificant. For the "Being" of which it speaks is not properly an object of worship, but a logically expedient makeshift that serves the purposes of *ad hoc* deductive convenience for the sake of an existence proof. *Non in dialectica,* wrote St. Ambrose, *complacuit deo salvum facere populum suum.*

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[3] A useful synthesis of recent discussions is contained in W. Kneale's paper "Is Existence a Predicate?", *Aristotelian Society Supplementary Volume 15* (1936) reprinted in *Readings in Philosophical Analysis* ed. by H. Feigl and W. Sellars (N. Y., 1949), pp. 29-43.

[4] See Chapter 8 of W. T. Stace's fine book *Time and Eternity* (Princeton, 1952). H. H. Farmer writes: "We might put it paradoxically by saying that if anyone succeeded in proving the existence of God he would by that very fact show that he had failed. For a God Who could be thus proved would not be the God under discussion, but something else. The word would in fact have changed its meaning in the course of the argument" (*Towards Belief in God* [N. Y., 1947], p. 30). Similarly J. Baillie holds that "It is not as a result of an inference of any kind, whether explicit or implicit, whether laboriously excogitated or swiftly intuited, that knowledge of God's reality comes to us. It comes rather through our direct personal encounter with Him . . . " (*Our Knowledge of God* [N. Y., 1939], p 143).

These three lines of thought appear to represent the main grounds for the current, virtually universal disrepute of the ontological proof. I propose here to show that, with appropriate alteration, it is possible to reformulate the ontological argument in such a way that these traditional objections (among others) are obviated altogether.

III. A Revision of the Ontological Argument

The leading idea on which the revision of the ontological proof which I wish here to present is based is the elimination of the argument's use of and dependence upon an explicit definition of the term "God" (e.g. as "the perfect being"). For one thing, the scholastic ideal of explicit definition of all terms *per genus et differentiam* has in modern logic long given way to a more diversified theory of definitions. The theoretical feasibility and pragmatic desirability of alternative procedures of definition is no longer questioned by logicians. And as to the theologians, there is a powerful and long-standing tradition of religious thought that denies altogether the possibility of presenting a satisfactory characterization of God in human language or in human terms (cf. for instance the Scholastic doctrine of analogical predication). There is thus also a strong presumption that a rejection of explicit definition is both warranted and plausible.

But if we give up the employment of an explicit definition of the term "God", how is the ontological proof, which appears to rest fundamentally and completely upon such a definition, to be maintained? To see how a reasonable answer to this pivotal question is to be had, it is necessary briefly to digress into the logical theory of definition, and to see also how it comes about that propositions can be true "by virtue of" definitions.

As has been adumbrated above, the theory of definition of current logic no longer has the simplicity and uniformity obtained by insistence that all terms be accorded an explicit definition *per genus et differentiam.* It has been recognized that many terms cannot be defined in this manner. Certain words, it is generally agreed, cannot be defined explicitly at all, but are such that their meaning can only be grasped on the basis, and within the context, of experience. Specifically this category includes, *inter alia*, those words which themselves designate experiences or aspects of experience: perception words (names of colours, odours, etc.), sensation words (aches, pains, etc.), feeling words (alacrity, lethargy, etc.), emotion words (anger, gaiety, etc.). Such words, which characterize the modes and character of immediate experience, cannot be provided with an explicit definition. The meaning of such words can be learned or indicated only experientially, by referring them to the experiences themselves. There is thus a category of terms which cannot be defined explicitly at all, but whose meaning can be conveyed only experientially.

These experience-presupposing terms cannot be understood by those who lack the experience that is relevant to them, just as a man blind from birth can have no concept of the colour red. This of course is not to say that a person who lacks the experience appropriate to such a term can have no information at all regarding the item for which it stands. A blind man can know that the word "red" applies to a kind of experience that he does not have, and that this experience takes place when certain physical and physiological transactions occur, etc., etc In short, he can have all sorts of information *about* seeing red But he cannot see red, does not know what it is like to see red, and so lacks the indispensable requisite for an adequate understanding of the term

One other point regarding these experience-presupposing terms is necessary. The experiences in question are, to be sure, themselves a wholly subjective matter whose contents cannot be communicated in full Yet there are perfectly objective *tests* as to whether or not an experience of the type in question has taken place For example, a person who would not assent to "Red appears different from blue", or who would not demur from "Orange is less similar to red than to green", would thereby furnish us with evidence of gaps or lacks in his colour experience. While we cannot examine other persons' experiences, we can obtain substantial information regarding them by noting how the relevant experience-presupposing terms are applied and employed by them.

On this account, the status of a proposition like "Red is more similar to orange than to green" is in effect a synthetic *a priori* truth. It is synthetic in that it presupposes experience, namely that fundamental experience, indispensable for having a proper grasp of the very *meaning* of the key terms involved.

Thus, since it is presupposed for the *meaningfulness* of the proposition, experience is, *a fortiori*, involved in its truth. On the other hand, the proposition is *a priori* because its truth "follows" from its meaning in the sense that, once a body of experience adequate as a basis for a grasp of the meaning of the terms is given, this is *ipso facto* adequate as a basis for the truth of the propositions in question. It would be incorrect to construe a dissent from such a proposition as expressive of disagreement on the facts, for it is properly to be taken as *prima facie* evidence that its meaning was not correctly understood, i.e. that there exist gaps or deficiencies in the visual experience of the dissenter that preclude a correct understanding of the statements in question.

The reader has doubtless foreseen what is to come. I wish to submit that it is quite in order to apply all of these considerations regarding experience-presupposing definitions to the term "God". I propose to classify the word "God" within the category of words which cannot be given an explicit definition, but whose meaning can only be grasped within experience. On this view, a person whose history is devoid of religious experience simply cannot have an adequate grasp of the meaning of the word "God". And I submit that a person who *does* have a basis of experience adequate to an understanding of this term, also has, *ipso facto*, the experiential basis of evidence adequate to underwrite rational, warranted assent to the proposition that God exists.

Thus God's existence does, after a fashion, "follow" from the definition of the term, not in any deductive sense, but in the sense that that body of experience indispensable to an adequate understanding of this (experience-presupposing) word is necessarily also adequate to validate reasoned assent to this proposition. Thus the proposition "God exists" is, on the present concept, "inescapable in the sense that it is self-evident, that its truth or validity could [I say *must* — NR] be grasped by someone who thought clearly and thoroughly understood the terms involved, without the necessity of deducing it from other propositions".[5] In consequence, a dissent from this statement is *prima facie* evidence of failure to grasp the meaning of the term, and indicates a lack in the religious experience of the person in question. From this standpoint we mark the propriety and correctness of J. J. C. Smart's perceptive observation: "I suggest, the question 'Does God exist?' has no clear meaning for the unconverted. But for the converted, the question no longer arises. The word 'God' gets its meaning from the part it plays in religious speech and literature [I would add *experience* — NR], and in religious speech and literature the question of existence does not arise."[6]

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[5] G. E. Hughes in *New Essays in Philosophical Theology*, ed. by A. Flew and A. MacIntyre (London, 1955), p. 63.

Let us contrast this revision of the ontological argument with the classical version of Descartes. No longer is the purported attribute of existence held to be included among the essential, defining attributes comprising an explicit definition of the term "God". And therefore the statement that God exists is no longer held to be analytic, nor is its denial logically self-contradictory. Rather, the line of reasoning now is: (*a*) The word "God" is not susceptible of an explicit definition but is a term whose meaning can only be had on the basis of religious experience. (*b*) A body of experience adequate as a basis for an understanding of this term must also be adequate as an evidential basis for assent to the proposition that God exists.[7] Therefore, (*c*) a denial of God's existence is indicative of a failure to grasp the meaning of the word.[8] On the basis of this revision, then, the statement "God exists" is no longer analytic, but is synthetic *a priori*. It is synthetic *a priori* in just the same sense as such statements as, e.g., "Orange is more similar to red than green". The meaning of such a statement presupposes experience, and thus its truth does so *a fortiori*, so that the statement is synthetic. But the evidential basis for its truth does not call for any experience *over and above* that demanded by its meaning. Therefore the truth of the assertion can be said to "follow" from its meaning, assuring the *a priori* status of the statement.

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[6] P. 41 of his essay "The Existence of God" in *New Essays in Philosophical Theology*, ed. by A. Flew and A. MacIntyre (London, 1955), pp. 28-46.

[7] In his essay "A Religious Way of Knowing" (*New Essays in Philosophical Theology*, ed. by A. Flew and A MacIntyre [London, 1955], pp. 76-95), C. B. Martin argues that religious experience can never provide a basis for asserting God's existence on the grounds that "the addition of the existential claim 'God exists' to the psychological claim of having certain experiences must be shown to be warrantable" and that this cannot be done. This argument misfires because experiences themselves can suffice as warrant for the existential theses based upon them, and, as Berkeley so elegantly argued, an extra-experiential link is never forthcoming as a bridge of guarantee between experience and its objects, even in sense-perception.

[8] St. Anselm's original presentation of the ontological proof makes use of essentially the same conception Anselm writes: "But how has the fool, [who says in his heart 'There is no God' (Psalms LIII 1)] said in his heart what he could not conceive, . . . since it is the same to say in the heart, and to conceive. But . . . there is more than one way in which a thing is said in the heart or conceived. For, in one sense, an object is conceived, when the word signifying it is conceived; and in another, when the very entity which the object is, is understood. In the former sense, then, God can be conceived not to exist; but in the latter, not at all. . . So, then, no one who understands can conceive that God does not exist; although he says these words in his heart, either without any, or with some foreign, signification." (Quoted from A. Fremantle, *The Age of Belief*, N. Y , 1954).

The line of thought here put forward is a rejection of the practice of philosophers who, behind Hume's standard, hold that a belief in God rests on *faith*, where "faith", they insist, is to be construed as a belief that is not justified or supported by evidence. In *this* sense, "faith' does not enter into the matter at all. For an intelligent and comprehending assent to the proposition that God exists can on the present account, be forthcoming only where there is a foundation in religious experience to render meaningful the proposition in question, and such experience, if adequate to this task, must *also* provide a suitable body of supporting evidence for the statement.

IV. The Objections Reconsidered

With this proposed revision, the ontological proof is at once freed from the three traditional objections we have considered above. First, the proof is no longer founded upon the supposed status of "existence" as a predicate, i.e. an attribute-denoting term. In giving up the use of an explicit definition of "God", we also abandon the procedure of including existence among the defining attributes which comprise the intension of this term We have shifted the argument on to a very different logical basis. For it is held, not that existence is an essential, defining attribute of God, but that the experiential foundation indispensable to an understanding of the term "God" must *ipso facto* provide adequate grounds for assent to the proposition that God exists.

The second objection, that the deductive character of the ontological proof necessitates an assumption in the premisses of the very point at issue in the conclusion, is also averted in the revision. The modified proof here presented no longer depends on the deductive consequences of an explicit definition, but on the fact that the experiential basis indispensable for an understanding of the term must inevitably comprehend the evidential grounds for assent to certain propositions in which the term occurs. In the face of a dissent from "God exists", as in the face of dissent from "Yellow is lighter than purple", we are forced to a like interpretation, *viz.* that the key terms in question are not properly understood. And, of course, to say this is not by any means the same as the circular procedure of claiming that the truth of these statements follows deductively from the formal definitions of the terms in question. Indeed, on the present view these terms have no explicit definitions, and can only be "defined" experientially.

Finally, the third traditional objection, *viz.* that the ontological proof hinges upon interpreting the word "God" in a religiously trivial and irrelevant sense, also falls to the ground For the revised proof depends pivotally upon the thesis that the term is not defined in any logically *ad hoc* manner whatsoever, and that its meaning is not to be caught within the net of any pat formula. In the context of the proof, as in the living religions, the meaning and significance of the name of the Deity derives from a relationship to the sphere of man's religious experience.

I wish also to observe that the proposed revision of the ontological proof not only secures it against the well-conceived objection advanced by St Thomas Aquinas, but indeed is actually in close accord with St. Thomas' analysis of the problem.

> "No one can mentally admit the opposite of what is self-evident, as The Philosopher states concerning the first principles of demonstration (*Metaph.* 1005 b 11). But the opposite of the proposition *God is* can be mentally admitted. *The fool said in his heart, There is no God* (*Psalms* LIII, 1) Therefore, that God exists is not self-evident. . . . . . Now because we do not know the essence of God, the proposition [that he exists] is not self-evident to us, but needs to be demonstrated by things that are more known to us, though less known in their nature — namely, by His effects."[9]

St. Thomas thus rejects the ontological proof on the grounds that "God exists" is not analytic (i e. "self-evident") since its denial is no contradiction, but an intelligible assertion. However, St. Thomas' analysis does not stop here, but probes more deeply into the concept of self-evidence.

> "A thing can be self-evident in either of two ways: on the one hand, self-evident *per se*, though not to us. on the other hand, self-evident in itself, and to us. A proposition is self-evident because the predicate is included in the essence of the subject: e.g., *Man is an animal,* for animality is contained in the essence of man. If, therefore. the essence of the predicate and subject be known to all. the proposition will be self-evident to all . If, however. there are some to whom the essence of the predicate and subject is unknown, the proposition will be self-evident *per se,* but not to those who do not know the meaning of the predicate and subject of the proposition Therefore, it

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[9] *Summa Theologica,* I, ii, 2, 1

> happens, as Boethius says, that there are some notions of the mind which are common and self-evident only to the learned . . . . Therefore I say that this proposition, *God exists,* is self-evident *per se,* for the predicate is in the subject, because God's essence *is* His own existence as will be shown below. But because we do not know the essence of God, the proposition is not self-evident to us."[10]

What St. Thomas is here asserting is that a sound understanding of the term "God" would in fact entail assent to the proposition that God exists, so that this proposition is self-evident *per se,* but that this proposition is none the less not self-evident *simpliciter* because not only is the term "God" not among the "common notions of which no one is ignorant", but indeed "we do not know the essence of God". To this position, the present recasting of the ontological argument requires solely the addition of the thesis that *some* men do have, on the basis of their religious experience, an understanding of this essence, which, albeit partial, can suffice to make the proposition that God exists self-evident *for them* (just as, according to Boethius, some propositions are self-evident only to "the learned"). The present revision of the ontological proof is therefore in rather close agreement with the analysis of St. Thomas

V. A Shortcoming?

The revised "proof", not being a formal, logically valid proof, lacks the power of compelling rational assent. If a correct and logically strict proof of God's existence, from wholly unobjectionable premises, were forthcoming, and this is the claim of the ontological argument in its classical formulation (cf. our motto from St. Anselm), it would provide a great theological weapon. For it would mean that rational persons could no longer withhold assent to the proposition that God exists, and would thus be a decisive means of refuting the sceptic and converting the unbeliever. Such persuasive force is wholly lacking in the revised version of the ontological argument here proposed. Since it sets up religious experience as a prerequisite for the argument, it is eminently unsuited as a means of confounding the atheist, sceptic, and agnostic. Thus the argument in its revised form is capable of carrying persuasive force only for those who actually do not stand in need of persuasion. With the proposed revision, the ontological argument has wholly lost its status as a proof. If I *demonstrate* the existence of a prime greater than any designated integer that is given, I can charge a person who persists in denying the existence of a prime greater than 10^{10} with a lack of intelligence or logical acumen. But no such change can be brought against one, who, in the face of the present recasting of the ontological argument, continues to deny its conclusion. (The only charge that we lodge against him is that of a lack of religious insight and experience, a lack which *also* precludes him from a sound understanding of the word "God" as used by religious people.)

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[10] *Ibid*

I must confess at once that I see no way of refuting such an objection to the revision on the grounds of a loss of demonstrative force. However, in point of fact it is not properly an *objection* at all, but an inevitable recognition of a fundamental epistemological fact. There is, and can be, no way of *demonstrating* existence and there is no escape from Hume's thesis that only experience can provide knowledge of matters of fact and existence. Religious knowledge must derive from religious experience. In religion, as in science, there is no viable alternative to empiricism, for God cannot be demonstrated by logic. Logical manipulations cannot successfully be employed for theological purposes as a surrogate or shortcut for religious experience.

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DISCUSSION

POPPERIAN CONFIRMATION AND THE PARADOX OF THE RAVENS

By D. STOVE

(*a*) Mr. J. W. N. Watkins has recently[1] tried to use the well-known paradox of the ravens to support Popper's view that instantial confirmation cannot accredit or render "probable" a universal generalisation. I do not think he has succeeded.

Watkins sets the paradox out as follows.

"Each of the following propositions seems intuitively acceptable: (1) Observations of black ravens confirm 'All ravens are black' . . . (2) Observations of black shoes, white swans, etc. are neutral to 'All ravens are black' . . . (3) If observations confirm one formulation of a hypothesis they confirm any logically equivalent formulation."[2] Yet not all of these can be true, for (1) and (3) entail that non-black non-ravens confirm "All ravens are black", contrary to (2). Watkins points out, furthermore, that if those white swans had been non-white, those shoes non-black — i.e. if the evidence had been quite contrary to what it was — it would still have instantially confirmed "All ravens are black" by confirming "Everything is not raven or black".

Watkins then writes: "We thus have the outrageous situation where a mass of evidence confirms a universal hypothesis and disconfirms its existential denial although a mass of flatly *conflicting* evidence would equally confirm the hypothesis and disconfirm its denial. The conclusion must be that the colossal inflation of instantiating evidence generated by Hempel's unquestionable equivalence-condition [(3)] is utterly irrelevant for the genuine confirmation of a universal hypothesis. Since an empirical universal hypothesis is instantiated by every object whatever except those whose existence it prohibits, it follows that, for its empirical confirmation, instantiation counts for nothing, and that only attempted dis-instantiation, attempted falsification, matters."[3]

(*b*) Let us, then, substitute for (1) above another condition, (W.1.): "Observations which fail to falsify 'All ravens are black' confirm it."

But of course this does absolutely nothing to mitigate the paradox! For all those black shoes, white swans, non-black shoes, non-white swans, etc, re-appear as observations which fail to falsify "All ravens are black"; and evidence which is effortlessly accumulated, enormous in bulk, and conflicting, accrues just as much as before to this or any universal hypothesis.

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[1] "Between Analytic and Empirical", *Philosophy* April, 1957. Except where indicated, all my references are to this article.

[2] p. 116.

[3] p. 116. Italics in text

(*c*) But it will be said that I am neglecting the provision that the observations which fail to falsify the hypothesis are to be observations made in *tests* of the hypothesis, i.e. in deliberate attempts to falsify it.

This provision is meant to be all important[4]. With it, Watkins is obviously right in saying that the policy of accrediting universal hypotheses by attempted falsification evades the embarrassment of "paradoxical" confirmation. But whatever "tests" are, a corresponding provision will just as obviously salvage the policy of accrediting universal hypotheses by instantial confirmation. For certainly no one would attempt to falsify "All ravens are black" by observing swans, shoes, etc.; but neither would anyone attempt to instantially-confirm it by such observations

Thus if we amend (W.1) to read "Observations which are made in tests of 'All ravens are black', and which fail to falsify that hypothesis, confirm it", we must in fairness amend the original (1) to read "Observations of black ravens which are made in tests of 'All ravens are black' confirm that hypothesis". And now both accounts of confirmation evade the paradox, superficially at least.

It should have been obvious that what corresponds to "failure in attempts to falsify" is "*success* in attempts to instantially-confirm", and that it is completely tendentious to compare instantial-confirmation-paradoxical-or-non-paradoxical with non-paradoxical-failures-to-falsify. Once we make the appropriate qualifications by means of "tests", "attempts", and the like, the paradox cannot favour one policy against the other. (If what I say in (*d*) below is right, neither policy really evades the paradox.)

(*d*) Until we notice the tendentiousness of this comparison, we are bound to be led by Watkins' argument to think that the degree to which an hypothesis is accredited by observation somehow depends on the amount of effort that has been expended upon it; or at least that there is some logical connection between there being evidence for an hypothesis and efforts having been made to test it. Now we know that this is false, and such a consequence should arouse suspicion about an argument which leads to it. But Watkins actually embraces this conclusion, for he maintains that in order to accredit a hypothesis evidence must be "observed by deliberate intention, and not, so to say, stumbled upon by mere good fortune"[5].

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[4] Watkins italicises "tests" on pp 118 and 120

This seems to me a perfect *reductio ad absurdum* of the denial of a logic of induction. We know, of course, that effort — in searching, in contriving experiments, in checking initial conditions, in persisting in all of these and more — is often *causally* necessary and sometimes *causally* sufficient for the getting of evidence. But to suggest that effort can be *logically* even necessary, let alone sufficient, for there being evidence is to replace the whole question of the principles of inductive or scientific evidence by a question of the history and psychology of individual scientists. The *attempts* that might be made to falsify or to instantially-confirm a hypothesis are quite irrelevant to the weight of the evidence, if any, resulting from such attempts. It is the observations that constitute the evidence, not the attempts to make those observations.

If nature were kinder to scientists than she is, crucial experiments would abound without human contrivance; if she were much less kind, all the ingenuity of scientists' attempts to accredit hypotheses would count for nothing. But the principles of evidence can lay claim to objectivity and generality only if they are logical and hence independent of what nature is like. When we speak of a particular test of an hypothesis as "severe", for example, we mean, surely to say something about the mutual logical bearing (including probability-logical) of various statements: *viz.* that the joint probability of the hypothesis being false and the result predicted by the hypothesis being true is much lower than the probability of the corresponding conjunction prior to this test. Whether we find it hard or easy to discover or contrive tests that answer to this description is nothing whatever to the purpose.

If we were to accept Watkins' idea that observations count as evidence only when they are deliberately made, we should never be allowed to say, what we quite often do say, that new and weighty evidence for a hypothesis has turned up in some quarter where no one had thought of looking for it. Of course, this disadvantage will be minor if we are to be allowed in return to say that the hypothesis that all ravens are black cannot be refuted by non-black ravens merely "stumbled upon"!

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[5] "Confirmable and Influential Metaphysics", *Mind* July 1958, p 316

KANT AND THE COSMOLOGICAL ARGUMENT

By Peter Remnant

It is frequently said that Kant has shown that the cosmological argument for the existence of God presupposes the validity of the ontological argument, to the embarrassment of all those who accept the validity of the former but deny the validity of the latter.

However, it has been recently maintained by several writers that Kant is guilty of a simple logical howler in the course of his demonstration — in the words of one of these writers, that Kant's reasoning "is a piece of sheer though, of course, unconscious sophistry"[1].

I shall begin by stating Kant's argument. Kant represents the cosmological argument as comprising two parts: first, the argument that since something exists therefore a necessary being exists; second, the argument that if anything is a necessary being then it must be an infinitely perfect being, that is, God. He does not, as Father Johnston says he does, attempt to reduce the cosmological argument to the ontological argument. What he does do is argue that the second part of the cosmological argument presupposes the ontological argument. I shall restrict myself to what Kant has to say in defence of this contention.

If we accept the first part of the cosmological argument as sound and draw the conclusion that a necessary being exists we must then show that this necessary being possesses the properties which theologians attribute to God. But, although the cosmological argument begins with an appeal to experience, it "uses this experience only for a single step in the argument, namely, to conclude the existence of a necessary being. What properties this being may have, the empirical premiss cannot tell us. Reason therefore abandons experience altogether, and endeavours to discover from mere concepts what properties an absolutely necessary being must have . . . "[2] Reason concludes that only an

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[1] T. A. Johnston, S. J., "A Note on Kant's Criticism of the Arguments for the Existence of God", this *Journal*, 21 (1943), p. 11. *Cf.* J. J. C. Smart, "The Existence of God", in Flew and MacIntyre, *New Essays in Philosophical Theology*, pp. 36-37. Can sophistry be unconscious? In view of the prevailing tone of Father Johnston's paper the concession must be taken for what it is worth.

[2] Kant, *Critique of Pure Reason*, A606, B634. All references are to the Kemp Smith translation. Father Johnston has some harsh things to say about the quality of contemporary scholarship, but it is curious to find anyone still relying, as he does, on the old Meiklejohn translation.

ens realissimum could be a necessary being. But to draw such a conclusion is, Kant says, tantamount to carrying out the ontological argument.

This he seeks to show by setting the matter out formally. We wish to prove that a necessary being, any necessary being, must be an *ens realissimum.* Now if it is the case, as we hope to be able to establish, that any necessary being will be an *ens realissimum,* then, converting *per accidens,* some *entia realissima* will be necessary beings. "But one *ens realissimum* is in no respect different from another, and what is true of *some* under this concept is true also of all.' (A608, B636.) Therefore every *ens realissimum* must be a necessary being. This means that "the mere concept of the *ens realissimum* must carry with it the absolute necessity of that being; and this is precisely what the ontological proof has asserted . . " (*Ibid.*)

It should be clearly noted that "A necessary being must be an *ens realissimum*" is not, according to Kant, the *conclusion* of the first part of the cosmological argument. It is what still remains to be proved after completion of the first part of the argument. Failure to grasp this point has led another writer to attribute to Kant the bizarre argument that, since the conclusion of the cosmological argument is the converse of the conclusion of the ontological argument and "since the truth or falsity of the convertend entails the truth or falsity of the converse, it follows that *a being which must exist is perfect* (the conclusion of the argument from contingency) is no truer than its convertend, *a perfect being must exist* (the conclusion of the ontological argument)."[8]

The cosmological argument then, according to Kant, presupposes the ontological argument in the sense that the cosmological argument depends for its final step on the principle that any necessary being must be a perfect being; this principle entails the proposition that any perfect being is a necessary being; and not only is the latter principle the conclusion of the ontological argument, but the ontological argument constitutes the only even plausible attempt so far to establish the truth of this proposition. It should be noted, however, that even given the conclusion of the ontological argument we cannot deduce from it the principle needed by the cosmological argument without the aid of the further principle that whatever is true of some necessary being is true of all. In these exalted realms it is difficult to know which principles are self-evident and which need proof.

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[8] Gerard Smith, S. J., *Natural Theology, Metaphysics II.* N. Y., Macmillan. 1951. p. 69 Father Smith does not raise any objection to this argument as such, but denies that the one conclusion is the converse of the other.

So much for Kant's demonstration. On what grounds do Father Johnston and Professor Smart reject it as fallacious? Here is Father Johnston's criticism.

> "When he [Kant] goes through the process of converting the proposition 'Necessary being is infinitely perfect being', he takes it for granted that it is *by this very proposition* that the existence of necessary being is established, entirely neglecting the fact that the existence of necessary being has already been established by the previous step in the argument. When we say 'Necessary being is infinitely perfect', we are speaking of a being already known to exist."[4]

This is the whole of what Father Johnston has to say with regard to the fallacy in Kant's reasoning; he does not say why he thinks that Kant is taking it for granted that the existence of necessary being is established by the proposition in question. But it would be curious if Kant had forgotten already what he had written on the previous page, where the problem is clearly stated — Given that we have proved the existence of necessary being, how can we prove that this is the *ens realissimum?* — and particularly curious since, as Smart points out, the conversion of "Any necessary being is infinitely perfect" to "Some perfect being is necessary" is valid only if a necessary being exists.

Smart's criticism adds nothing substantial to that of Father Johnston: Kant has forgotten that the existence of a necessary being has already been proved; he has been taken in by the ambiguity of propositions of the form "All As are Bs", which may mean either "There are As, and they are all Bs", or "If there are any As then they are all Bs"; Kant "has taken himself and other people in by using 'all' sometimes in the one way and sometimes in the other".[5]

I can see no force whatever in these criticisms — there is no reason to believe that Kant has forgotten or been taken in by anything in the course of his demonstration. Granting for the sake of argument that the first part of the cosmological argument has proved the existence of a necessary being he then asks: "How can it be proved that a being of this kind must be supremely perfect?" and he replies: "To prove that a necessary being must be a perfect being amounts to proving that a perfect being must be a necessary being — and this is just what the ontological argument, and no other known argument, is intended to do." It is irrelevant that in this case we already know that there is a necessary being — the ontological argument is not designed to prove *simpliciter* that there is a necessary being, and it is not assisted in arriving at its own proper conclusion by the knowledge that there is a necessary being.

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[4] Johnston, *op. cit.*, p. 15 Italics in the original.

[5] Smart, *op cit*, p 37

That this is the line of Kant's reasoning is borne out quite clearly by what he says in his introductory remarks to the discussion of the three theological arguments:

> " . . . If we take the issue as being that which is here stated, namely, *first,* that from any given existence . . . we can correctly infer the existence of an unconditionally necessary being; *secondly,* that we must regard a being which contains all reality . . . as being absolutely unconditioned, and that in this concept of an *ens realissimum* we have therefore found the concept of a thing to which we can also ascribe absolute necessity — granting all this, it by no means follows that the concept of a limited being . . . is for that reason incompatible with absolute necessity." (A588, B616.)[6]

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[6] Kemp Smith gives "reality" as the last word of the quotation. This is obviously a slip on his part—the German reads "Notwendigkeit"

CRITICAL NOTICE

THE ILLUSION OF THE EPOCH. By H. B. Acton. London: Cohen & West, 1955. 278 pp. 18s. net (U.K.)

Professor Acton has not succeeded in justifying his conclusion that "Marxism is a philosophical farrago" or, in general, in throwing fresh light on Marxist doctrine. A study which would bring out the real merits, as well as the real difficulties, of Marx's position would not follow the line indicated by Acton's sub-title ("Marxism-Leninism as a Philosophical Creed") and treat secondary figures like Engels, Lenin and Stalin (!) as clearing up matters which Marx leaves doubtful; nor would it confront Marx with merely commonplace views of "freedom", "right", "the individual", "the external world" and so on (all the bad pennies of philosophy) instead of exhibiting in detail Marx's case for the "illusory" character of such conceptions. A great deal of work is needed for this, Marx's directly philosophical expositions being neither extensive nor free from confusion; but to throw in the much greater confusions of his successors is no substitute for the positive and illuminating doctrines which a critical re-statement of his arguments can bring out. The "illusion" Acton refers to appears to be that of wholesale planning (human life guided and controlled down to the last detail) to which Marxism is in no way committed; his own illusions, unshaken by the impact of Marxism, are those of a vague "social betterment" and of a "speculation", a rumination on "first principles", which is not merely unhistorical but anti-historical. It is this "starting from scratch", characteristic alike of present-day Communists and of their opponents, which might properly be called "the illusion of the epoch".

Lenin, of course, in *Materialism and Empirio-Criticism,* was not setting up as a philosopher; his point was that even a non-philosopher could show that the various "empirical" views which were being canvassed in the party were in conflict with the doctrines of Engels and therefore with a true revolutionary line. His non-expertness comes out both in his hammering on the crude "copy" theory of knowledge and in his constant recourse to accusations of "fideism" (or deviation into *religious* views). But such crudities give no ground for saying that Marxism as such has no adequate reply to "phenomenalism" or for taking it to have a close affinity with "positivism" — as Acton does largely on the strength of its being anti-religious. It is true that Marxism is positivist in the sense of denying all "higher realities", but what is particularly being denied is anything of a supra-historical character, whether "religious" or otherwise, and, in viewing reality as historical, Marxism has little in common with any of the doctrines that have been called "positivism".

Again, Acton shows no awareness of the quite pedestrian character of Stalin's thinking and, while recognising that he also echoed Engels, seems to regard him as a thinker of considerable originality (apparently even, on p. 156, crediting him with the notion of large-scale industry) and treats as important (p. 260) his crude observations on language. 'A language, he argued, must be the same for a whole society, for proletarians as well as for bourgeoisie, if communication is to be possible and the society is to hold together. Whereas the change from feudalism to capitalism and from capitalism to socialism involved sudden breaks or leaps, language develops gradually and in independence of such changes in the economic basis, although, of course, the vocabulary is affected by them." Such a view (which Acton takes to show the inadequacy of the distinction between "basis" and "superstructure" in social life) implies no appreciation at all of the Marxist and historical view of things according to which language must develop as a feature of social activities and must vary with different ways of life, just as it is broadly recognised to do between the "learned" and the "vulgar" (one of the many points ignored by the contemporary partisans of "usage"). It should be understood, of course, that classes for Marx do have communication with one another and are not, as Acton in various places seems to imply, in conflict on all points (are not separate "nations"); but still they have a diversity of outlooks and hence of modes of expression, the extent of their communication is limited, even if they are roughly said to speak "the same language" and are parts of what is called a single society. In fact, if one were looking for something "socially neutral", something cutting across diverse forms of social activity and organisation, language would be a particularly bad example to choose. To do so would be to reduce it to an instrument of commerce or, at most, of production, instead of recognising it as a repository of the history, of the social movements, of its users But, even on an instrumental view of it, it could not be treated independently of "the economic basis of society" or of economic organisation generally.

For Marx, then, any question of human affairs is to be considered in terms of types of activity or ways of living, and a theory of knowledge in particular will have content only as a theory of interrelated activities. And this makes Acton's long-drawn-out discussion of "phenomenalism" beside the point. Even if Engels replaces the Hegelian conception of "reflection" by a Lockian representationism (as Lenin clearly does), this is alien to Marxism and to a *historical* view of knowledge — a view which permits learning or the acquisition of knowledge. Certainly Engels cannot show how "practice" can break down the barrier between inner and outer, if once this division has been set up. But the point is that Marx denies the division; the position he puts forward in the first of the *Theses on Feuerbach,* particularly, is that human sensory activity is "the active side" of the sensuous or, in a more recent terminology, that the object is the content of the mental process. There are obvious difficulties in this — Marx indeed seems to infer, from the fact that knower and known are both activities, that they are the same activity — but, without drawing this inference, we can still say that their being both activities enables them to be continuous with one another, with no intervening barrier, and that this continuity, the way in which mental and non-mental processes can be embraced in a single process or can "belong together", is the positive content of the doctrine of "reflection" and is explicitly set against any doctrine of mere *confrontation* (or of mere "data") which would make knowing impossible. Marx's position here is also opposed to the contention of Lenin that matter has a primary and mind a secondary reality, since mental processes are material (in the sense, not of being composed of some common substance, which is not true even of different non-mental things, but of being *space-filling* — apart from which the continuity referred to would not exist); though there is nothing in this materialist position incompatible with the view that, in any region in which mental processes exist, non-mental processes have *preceded* them. The inability to escape "phenomenalism", then, does not apply to Marx, since he is at the materialist standpoint (is raising questions of actual processes) at all stages.

The general position of the *Theses* is that to have true knowledge is to be moving with the movement of things, which is a revolutionary movement; it is only the revolutionary, participating in that movement, who really understands it — who has a "dialectic" understanding (i.e., precisely *participatory,* going beyond himself) as contrasted with the idle speculation of the non-participant. Apart from the major difficulty of the unification of all movements (something which is really incompatible with a historical logic or philosophy of process), there is the minor difficulty of showing how mere speculation, as non-involvement in reality, is even possible. But it is something quite different (and quite indefensible) to treat Marx as rejecting "speculation" in the sense of the inquiry into reality and demanding a concentration on "science" in the sense of the formulation and search for verification of hypotheses in some particular field. It is easy enough to pick texts which appear to be anti-philosophical, but a person who calls his doctrine "material*ism*" is certainly not repudiating philosophy. What Acton is doing is reading his own view of science, and his own view of "formal logic and dialectic", into Marxist doctrine. The position is not that "the laws of logic somehow copy or 'reflect' the real world" (p. 104) — or again that first principles or "fundamental notions" can be considered apart from concrete inquiries and activities; anything that exists (mental process as much as non-mental) is in "the real world" and there is no "external world" or "material world" from which some other region could be distinguished and set apart. The scientist is grappling with the only reality there is, and since it is dialectic or revolutionary he has to be a logician and dialectician — in other words, a philosopher. There is much to be said in criticism of the *Theses* — especially with regard to the line of unification in which what environs society is treated as itself social, so that all reality is social reality, all movement the social revolution — and in bringing out, in the general "materialist" position the oscillation of "material" between the meanings of *substantial* (being composed of the old "universal stuff) and *social* or, more exactly, *productive,* constituted by co-operative activity. But there is nothing in all this to suggest that Marx took a narrow procedural or instrumentalist view of science

Even if the method of the "natural sciences" were properly described as that of "hypothesis, observation and experiment" (p. 58), it would not be apparent how this was to be distinguished from the method of any other group of sciences; but, beyond that, it would not appear either how hypotheses came to be framed or how the procedure could break through to anything non-suppositional and thus be accorded "efficacy". If, in Marx and Engels, the test of "practice" is especially that of "touching and manipulating" (p. 50), this does not mean that tactual "data" are being given a special position, being taken to "break the barrier" in a way that other "data" do not; the point still is that there are transactions (contacts) not with "data" but with actualities, that there is no "judgment" or "interpretation" distinguishable from "data" in any field, and that without "manipulations" there can be no acquisition of knowledge, so that there can be no science distinct from "physical" science. It is not merely that in any knowledge there must be continuous processes embracing knower and known, but that there could be no connections between different *fields* (no possible attribution, e.g., if ethics were not a physical science, of an ethical character to anything that had a physical character) but simply separate "universes" or "worlds", each with its quite separate *knowers*. If, however, there *is* knowledge by the same knower of human and non-human facts, there must be continuity among them and they must all alike be historical or "physical"; as against the insoluble problem of relations between different kinds of reality, there is no difficulty at all about historical relations between historical things. This is the general point that "historical materialism" makes, and Acton does nothing in his criticism to show how the problem of discontinuity can be dealt with. Rather, he simply assumes that there can be facts of a lower or derivative kind (having the status of mere resultants) and facts of a higher kind (objects of "judgment", "values"); and this means, in particular, that for him there are no distinctively *social* facts and that he can only grope for Marx's meaning, can only give a series of inept *individualistic* interpretations of Marxian doctrine and find *it* incoherent because they are inadequate. That its very thesis is that coherence rests on the historical character of things, on the commensurability and interrelatedness of social, psychological and any other processes, would never appear from Acton's pages.

There are still, of course, obscurities and confusions in Marx's presentation of his view; in particular, if what can positively be made of "historical materialism" is to be brought out, a certain ambiguity in "historical" as well as in "material" must be noted. A historical view of things can be understood as a doctrine of reality as process, and this, as we have seen, is part of Marx's meaning. But there is also the more special meaning of "history" as *social* processes. And the "materialist" interpretation of history, in this sense, is the doctrine that productive organisation is what distinguishes social from other processes, is the "subject" of history or that which continues (though having distinguishable phases) while a society continues. It is in accordance with the Hegelian foundation of the doctrine of "subject" that social history should be identified with a postulated movement of things in general and that all its characters should be taken as embraced in a single conception — but such "totalism", as against pluralism (recognition of the distinction of any process from surrounding processes and of an irreducible plurality of processes within it) cuts at the root of concrete continuity and leads to forced interpretations and distortions. But it is still the case that only with the recognition of specific types of social activities and interactions can there be any social theory or any account of "history". And even if Marx confuses this particular subject with reality in general, he does make positive contributions to it — while at the same time showing us that such contributions can be made only if a philosophy of process is maintained and such unhistorical ("metaphysical") entities as "the ego" or "freedom" are discarded.

Mental and social activities, then, are processes among others, which they affect and are affected by, having the same status, the same regularity and predictability. But there is no question of *total* prediction or of a single "course of history". It is just because a strictly materialist view is pluralist as well as determinist that Marx's doctrine of single-track social development is a departure from it and forces him into such further inconsistencies as the doctrine of "shortening the birthpangs". The activity of a revolutionary party is in the same condition of interchange (regular transactions) with its surroundings as any other process, and if there were a single course of social activity, the party activity would have to be *part* of it — to be seen in its place within an inevitable progression — and could not come from outside and expedite it. But to note such inconsistencies is not to answer a materialism of a non-totalistic kind, of which Marx laid the foundation in his doctrine of social struggles throughout history. Thus when Acton says (p. 185) that we "deceive ourselves with almost empty phrases if we suppose that we can make predictions about such things as societies, civilisations, revolutions, classes, social orders, and constitutions, as we can about men, genes, gases, and stars", he is not showing how in that case we can talk about the former at all or what possible relation we can take them to have to the latter if they do not have the same kind of reality, and he is passing over or denying the fact that we can talk about a man, e.g., only as a complex of interrelated processes (a subject of internal and external "exchanges") and is blind to the fact that there is no greater illusion (no greater "emptiness") in the modern period than that of the "ego" standing above and directing, instead of *being*, processes. It is just this sort of thing that Marx is specially attacking in the *Theses*, which are an indictment of modern (Cartesian, egoistic) philosophy, and in the whole materialist conception of history.

There is no question, then, of social processes having a reality inferior to that of psychological processes; indeed, if we are to give any serious account of a man's life, we must present the interrelation (including the clash) of forms of activity in which he and others participate — of which he is a "vehicle" rather than an initiator. It is, for Marx, the development and sequence of forms of *productive* activity in particular that is social history (history in the usual sense), and this is only caricatured, and incidentally in an individualist way, in Engel's assertion that man must eat and drink, be clothed and sheltered, before he can engage in other activities. It would be tedious to go over the points at which, in his chapters on "Historical Materialism" and "Marxist Ethics", Acton comments on Marx in individualist terms and turns social questions into questions of someone's demands. But, to take one example, when he says (p. 112) that a "man's needs may be understood in the sense of everything he desires" (following this up, on p. 113, with the extraordinary statement that "we generally take it for granted that social science should find means for satisfying, not any and every desire, but *legitimate* desires"), he is missing the point that "needs" for materialism are what are required to keep certain things going and that these things are forms of activity, not "individuals". It is only in this way that there can be concrete questions, capable of being investigated, and thus that there can be social science — and not the loose guessing which is all that contemplation of X's choices, Y's choices, etc., can give rise to. In the same way, to Acton's observation (p. 155) that "the individuals who make up society do not get fused or transmuted, as in chemical combination", the answer is that forms of activity do get fused and transmuted, and so far from there being social atoms who can merely form a "mechanical mixture", it is such forms of activity that make up "individuals".

Illustration of this and connected points is obtained in Acton's treatment of "relations of production". He supports, as others have done, the view that the materialist interpretation of history is a technological interpretation, an explanation by reference to instruments of production — and, in this connection, that by reference to which an epoch is to be understood (its "basis") is taken as its cause or that which brings it about. The "causal" way of speaking is, of course, an easy one to slip into, and Marx also speaks occasionally as if his doctrine were a technological one ("the windmill gave us feudalism, the steammill capitalism", etc.); but, if such formulations were to be taken literally, Marx's whole distinction between "productive forces" and "relations of production" would be done away with. Acton speaks of the latter (p. 135) as "the ways in which men are related to one another as they operate the 'productive forces'", which suggests that it is simply a question of the interrelation of special operations within a general operation, of a complex manner in which energies are directed on materials — and such technical devices would still come under the head of "productive forces". As against any such differentiation of tasks within a single labouring group, what distinguishes "relations of production" is differentiation of *classes*, ways in which the different *factors* in production (land, capital, organisation, and not merely labour) are appropriated and made available for productive work, and secure their different types of return (rent, interest, profit, as well as wages). We find, of course, in different economies different combinations of these types of appropriation, but we certainly cannot describe any economy unless we take account of the forms of social procedure involved (whether or not we use expressions like "exploitation" in describing them), and thus unless we distinguish "relations of production" from "productive forces", however we take them to be connected. But, if classes are thought of merely as aggregates of "individuals", then it is natural to revert to ordinary "division of labour" and lose the major point.

Again, on p. 167, Acton says that his analysis of the notion of 'productive relationship' "has shown that this involves law, morals, and politics, and we can see that it is not fanciful to regard them as parts of the means of production". The former of these points is not denied but asserted by Marx; he is saying, in particular, that it is to a given factor in production that "rights" belong, that it is their "subject", that through which alone their character and history can be grasped, just as the productive process in general is the "subject" of the whole system or distribution of "rights". But the bearer of "rights" is still the function, the *provision* (which can be accomplished in various ways) of a certain factor to production, and not the provider as a separate "individual". And again it is not a question of the function's preceding the "rights" in time; there can be no system of appropriation of factors without a system of sanctions, however primitive, but Marx's point still is that it is from the former that the latter can be understood, and especially that there can be no concrete and historical account of law except by reference to its economic "basis", no intelligible treatment of it as something independent or "absolute", as carrying its own "principles" with it. No doubt it can be argued that

Marx takes too narrow a view of productive activity, that social creativeness cannot be reduced to industry (and emphatically, of course, cannot be understood as a mere matter of survival), but still the search for the continuing "subjects" of social processes is a condition of determining their regularities (laws) and of having an objective social theory. This cannot be obtained on the level of conceptions like "performing one's task" or "man, the tool-using animal" (these precise phrases do not occur in the text), but when he reduces the class theory of "relations of production" to a doctrine of the personal use of means of production, Acton is paring away all that sets Marx above that level.

Just as it is his individualism that prevents Acton from getting hold of the materialist view of society, so it is his moralism that prevents his grasping the materialist view of ethics. "The adjective 'moralistic' ", he says (p. 213) "is today used in spoken English to express criticism of exaggerated or misplaced moral judgments . . . More generally, the noun 'moralism' is used for an exaggerated or misplaced zeal for conventional moral rules." The materialist objection, however, is on the ground not of exaggeration but of error; it is a direct rejection of doctrines of "right" or "ought to be". No doubt Marxists are sometimes inconsistent here as on the question of freedom and can be found taking a preceptual line; but their main position still is that moral or ethical questions are questions of concrete social activity and that the notion of right (or required) conduct is misleading when, as it commonly is, it is put forward without specification of the activity for which it is required, the sort of life which the conduct keeps going — and that the explicit view that no question of such specification arises, that certain things are just "right", is a thoroughly confused one. This is not in the least to say that ethical activities of this or that kind cannot be investigated as much as anything else; it is only to say that such investigation is just as positive, just as much an inquiry into fact, as any other. It may be (p. 178) "commonly supposed that one important characteristic of the method of the natural sciences is to be free from any preconceptions about the value, the goodness or badness, the perfection or defect, of what is being investigated", or, more briefly, that natural science is "value-free". But the Marxian contention is that either goodness and badness are the subject of a natural science or there is no knowledge of them at all, that either "values" are natural facts or there are no "values". As previously argued, there will be no assertable connection between objects of observation and objects of "judgment" unless they can be taken in a single view, as parts of a single experience; and this means that that "judgment" which is supposed to be beyond the compass of natural science does not exist. There is no subject or argument in which "the moral beliefs of men", as contrasted with the occurrence of the objects of these beliefs, "are taken as data" (p. 66); the question is what concretely is believed or found to be fact, and to this question, or this line of Marxian attack, on "conventional moral rules" Acton has simply not addressed himself.

What he sets against Marxism is an "ethics" as empty as the doctrine of "the individual". This is illustrated on p. 184 by the notion of "physical evils", which can only mean certain physical things people want to avoid and which is paralleled by the notion of "goods" as objects of desire, neither notion *characterising* the objects in question. If there is anything that has an ethical character of its own, then the question of its nature and history can be gone into and ethics will be the positive science of ethical processes (more particularly, good activities) — it will be quite a side-issue whether anyone wants them or not, and there will be no question of the pursuit of them being "absolutely required" of anyone or of their being among "the principal purposes of human life" (p. 1). And, however incomplete Marx's account of the going on of such processes may be, it is no reproach or ground of objection to his position that he still recognises positive goodness while rejecting "moralism" or the "ethics" of categorical imperatives; while at the same time any deviations into moralism on his part do not affect the strength of his "materialist" view. It is those who take ethical assertions as something other than the assertion of certain historical occurrences who are really jettisoning the science of ethics

The doctrine of absolute "right' (of that which *in itself* is required or obligatory, i.e. of a relation lacking its full complement of terms) is an outstanding example of what Marx and Engels call "ideology". This is a question which Acton handles weakly, even taking as a serious development of theory the use of "ideology", by Stalin and others, in the commonplace sense of "ideas" or beliefs. Acton quotes (p. 126) the passage in which Marx and Engels say, "If in all ideology men and their circumstances appear upside down as in a *camera obscura,* this phenomenon arises just as much from their historical life-process as the inversion of objects in the retina does from their physical life-process", but, while taking this to assert that it is natural that men should get distorted views of "the world", he does not appear to grasp the force of *inversion.* The point is that A is taken to depend on B, when in fact B depends on A — and thus, not only that a certain line of conduct is taken to be *in itself* right or required when in fact it is required for the carrying on of certain social activities or the maintaining of certain social relations, but that it is taken to be what sanctions, or lays down the necessity of, these activities and relations. The "illusion of the epoch", then, is that such "sanctions" are absolute or primary instead of being relative or secondary (being incidental to "capitalist production", e.g.); and, while it is positive inquiry, and not something that is itself "ideology", that establishes such facts of derivation — that completes the formulae, misleading in themselves, by adding "while capitalism exists" or "if capitalism is to be maintained", etc. — it is clear that such inquiry carries with it a critical apparatus, that it involves a philosophical examination of forms and is not a mere collection of "data"; and this reinforces the point that Marx's positive doctrines cannot be separated from his philosophy, even if much that is called philosophy would be described by him as ideology.

The doctrine of "data", indeed, is just another example of ideological or "relativist" thinking — the postulation of "that whose nature it is to be given to us" (or that which has *givenness* in itself) is on a par with the postulation of "that whose nature it is to be required of us" (or to be obligatory), and the rejection of it exemplifies rejection of "natures" and the insistence on explanation in terms of concrete activities, of the interaction of processes all on the same empirical level. As before, Marx's "totalism" prevents him from bringing out this form of argument clearly and in detail, but it can still be said that this is the positive content of his doctrine of "ideology" in the same sense as that in which he himself exhibits the positive content of doctrines which profess to be independent of and indeed to stand above social processes. The vital consideration is still that of continuity, and it is this that Marx is upholding in his "materialism". In doing so, whatever his incidental errors, he has contributed notably to the philosophical criticism of dualism or the setting up of different kinds of reality, individualism or the postulation of any sort of atomic entity, and relativism, or the doctrine of constitutive relations, which is only one example of rationalism or the doctrine of "natures" as opposed to transactions, alike within anything and between it and other things.

These are the large issues that arise, but Acton is so fixed in his "separatism" — separation of "individuals", separation of kinds and "methods" of inquiry so that no connected view of things is possible — that he cannot arrive at a general view even of *Marx's* position but engages in piecemeal and pin-pricking criticisms. It is, I would maintain, the dualistic or divisive view that is a "farrago" instead of a philosophy. At any rate, it is the question whether there can be any "division in reality", especially whether anything unhistorical can be set against the historical, that specially calls for consideration which it does not receive in this book. Perhaps the main barrier to such unfettered discussion is Acton's inability even to conceive the possibility of the rejection as illusory of an "ethics" of right, carrying with it a moralistic treatment of all human affairs.

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REVIEWS

THE USES OF ARGUMENT. By Stephen Toulmin. Cambridge University Press, 1958. vii, 264 p. 22s. 6d. (U.K.)

(1) *Summary*

This book is described by its author as radical in intention but unoriginal in the arguments that it contains. It consists of five essays which are meant jointly to support the thesis that logic and epistemology should be replaced by a study which might be called comparative logic.

In the first chapter we are told that two arguments are in the same field if and only if the data of the two arguments are of the same logical type, and the conclusions are of the same logical type. Some features of arguments are field-invariant and some are field-dependent: the force of modal qualifiers such as "necessary", for example, is the same in all contexts, although the criteria for their use vary from field to field.

The second essay, which is on probability, is self-contained, but it is presented, together with the first, as being in part a preparation for the third essay in which "the crucial results of the inquiry are expounded". (p. 8.) Here it is contended that the traditional analysis of arguments has been crude, and a new layout, designed to reveal details previously obscured, is proposed. This leads to a distinction between analytic and substantial arguments.

In the fourth essay it is maintained that logicians, believing that logic might become a formal science like geometry, have taken as common and typical analytic arguments which are in fact most unusual, and that they have consequently lost touch with the business of assessing the substantial arguments that we use from day to day. In the final essay the philosophical consequences of this alleged error are sketched. In substantial arguments we often make type-jumps: we argue from the past to the future; from other people's behaviour to their feelings, or from what is to what ought to be. In such cases our arguments cannot be analytic, and if we identify soundness of argument with analyticity, we have to conclude that our arguments cannot be sound. Thus we come to believe that we never can argue cogently from the past to the future, or from behaviour to feelings, and we reach a state of puzzlement which is alleviated only when we see that substantial arguments may be sound although they are not analytic.

The criteria for the validity of substantial arguments vary from field to field and consequently the investigation of validity and other logical concepts will be a comparative study. Epistemology is to be divided between psychology and this new inquiry, and formal logic will remain beside the point. This last chapter and the one on probability are provocative, but, as the title of the book suggests, and as Toulmin says, it is the analysis of arguments that is central. In this notice, my remarks will be confined to this central topic.

(2) *The New Layout*

Toulmin does not claim that his discussion of arguments is exhaustive. He chooses to investigate the type of argument that can be put into the form: All X's are Y's, and S is an X. So S is a Y.

An argument is put forward to justify or to defend a claim, and it is by considering the lines of defence to which we retire when defending a claim that Toulmin gives his analysis of arguments. If we make the claim (C) that Harry is a British subject, we may be asked for the information upon which this claim is based. In reply, we offer the datum (D) that Harry was born in Bermuda. The connection may not be obvious, and for this or some other reason we may produce the warrant or inference licence (W) which justifies the step from D to C: in this case "A man born in Bermuda is a British subject".

A warrant may not justify the step from data to conclusion without qualification. "If a child has no brothers or sisters, it is likely to be selfish" does not warrant our passing from the datum "Harry is an only child" to the unqualified conclusion "Harry is selfish". In this case the conclusion that Harry is selfish must be qualified by the expression "it is likely that". Toulmin calls such expressions modal qualifiers (Q).

It may also be the case that a warrant is subject to exceptions. Given that Harry was born in Bermuda, we may argue that since a man born in Bermuda is *generally* a British sbject, Harry is *presumably* a British subject, *unless,* of course, he has become a naturalised American. Here "presumably" is a modal qualifier, and the "unless" clause is a statement of conditions of rebuttal (R).

At this stage our critic may accept that our warrant justifies our basing a suitably qualified claim upon our data, but he may question the warrant. In this case we produce the information upon which the warrant is based. This Toulmin calls the backing (B) for the warrant.

The simplest form of argument is "D, therefore C" or "D, so C", the C often being qualified in some way (Q). Unless we are challenged, or anticipate a challenge, the warrant (W) remains implicit. If W, having been stated, is questioned, we produce the backing (B). A line may be drawn here, for B is a factual statement, and, if challenged, it becomes a claim in defence of which we may produce another argument.

(3) *Modal Qualifiers*

Some of the modal qualifiers that Toulmin mentions or uses are "necessarily", "possibly", "probably", "presumably", "certainly" and "almost certainly". In one place he describes them as indicating "the degree of force which our data confer on our claim in virtue of our warrant" (p. 101), while in another he says that they indicate "the strength conferred by the warrant on (the) step" from data to claim. (p.101.) If we introduce the modal qualifier "necessarily" into an argument of the form

Since p, it follows that X is Y

we get either

(i) Since p, it follows that X is necessarily Y

or (ii) Since p, it follows necessarily that X is Y

depending upon which of Toulmin's two statements we follow. That this confusion is not trivial is indicated by the fact that there are values for "p", "X" and "Y" such that the same substitution in (i) and (ii) gives two different arguments, only one of which is valid.

My second point is that if we consider the modal qualifier "certainly" instead of "necessarily", then my first point cannot be made. "Since p, it follows that X is certainly Y" differs only in emphasis from "Since p, it certainly follows that X is Y". From this it appears that Toulmin has assembled under one heading expressions that are different from each other in a way that is important in logic: a conclusion that is confirmed by comparing "probably" with "necessarily" and with "certainly".

(4) *Form and Validity*

It is suggested that the difference between a major and a minor premise is so great that to call them both premises is scarcely justifiable. The claim or conclusion of an argument of the type under examination is a categorical statement of fact, and so is the datum or minor premise. The backing for a warrant will also be categorical, but the warrant itself is not. The warrant or major premise is hypothetical and general. "It is more than a repetition of the facts (of which the backing consists): it is a general moral of a practical character about the ways in which we can safely argue in view of these facts." (p. 106.) The nature of the backing for a warrant, and the relationship between it and the warrant vary from field to field. "The whale is a mammal", for example, is backed by a system of classification; "Anyone born in Bermuda is British" by statute, and Kepler's Laws by astronomical observations. Inasmuch as all these warrants are warrants, each of them has the force of a general hypothetical. But to go on to say that statements of the forms "The S is a P", "Any S is P", "S's are P's", and so on, are all really of the same form, is to ignore how, by our choice of words, we indicate the nature of the backing for a warrant. If we reduce all warrants to one or two forms, we obscure the diversity of their backings, and this we should not do because it is the backing that licenses an inference, the warrant merely showing what sort of inferences are licensed by the backing.

Sometimes one and the same sentence may be used to state a warrant, or to state the facts that back the warrant. Suppose we have an argument of the form: All X's are Y's, S is an X, so S is a Y, and suppose that the major premise has been established by complete enumeration. We may paraphrase the major premise in a way that emphasises its function as a warrant, or in a way that brings out its factual content. In the first case we have

(w) An X is certainly a Y
S is an X
So, S is certainly a Y

and in the second

(b) 100% of the X's are Y's
S is an X
So, S is certainly a Y.

Toulmin now says that when the argument is in form (w) "the parts of the conclusion are manifestly the same as the parts of the premises, and the conclusion can be obtained simply by shuffling the parts of the premises and re-arranging them". (p. 119.) This is true, but it is not the case, as Toulmin appears to suggest, that this is what is meant by saying that the argument is formally valid. Later on, he describes a warrant as being, in effect, a substitution rule (p. 210), but this is not sufficient either. These objections may seem pedantic: it might be replied that it would be easy for Toulmin to work out and to state which formal moves are permissible in arguing and which are not. This may be so, but to work this out would be to do some formal logic, and this activity is dismissed as being beside the point.

According to Toulmin any argument from D to C can be made formally valid by introducing a suitable warrant, but the step from D to C will be sound only if the warrant is properly established. The validity of an argument depends upon the warrant's being established, and its conclusiveness depends upon this and upon the truth of the data The warrant can be established only if it has the proper backing, and consequently it is maintained that "once we bring into the open the backing on which (in the last resort) the soundness of our arguments depends, the suggestion that validity is to be explained in terms of 'formal properties' in any geometrical sense loses it plausibility" (p. 120). It is not that the formal properties of an argument justify the step from the premises to the conclusion, but rather that the appropriate established warrant justifies the step from data to claim. Toulmin is right in emphasising the very different functions of major and minor premises in arguments of the type in question, but he is wrong in supposing that formal considerations are of no importance. He says that the validity of an argument cannot depend upon the formal features that are apparent when the argument is put into form (w) because the argument remains valid, or invalid, as the case may be, when it is put into the quite different form (b) But this is a *non sequitur.*

Three things are severally necessary and conjointly sufficient for the conclusiveness of an argument of the type that Toulmin is considering:

(p) that the warrant shall be formally appropriate,
(q) that this warrant shall be properly established,
(r) that the datum shall be true.

To these we may add a condition necessary for (q) and thus indirectly for the conclusiveness of the argument.

(q') that there shall be backing for the warrant. Toulmin talks as if (q') were a sufficient condition, and as if (p) were trivial, even though it may, strictly speaking, be necessary. "Provided that the correct warrant is employed", he says, "*any* argument can . . . become formally valid" (p. 119; my italics). But to investigate systematically which warrants will render a given argument formally valid is to do formal logic. Whether the warrants that have been established warrant the steps that we wish to take is a formal question, but answering it is not a mere formality.

(5) *Analytic and Substantial Arguments*

Consider the following arguments:

(iii) Petersen is a Swede. The proportion of Swedes who are Roman Catholics is zero. So Petersen is not a Roman Catholic.

(iv) This statue is made of bronze, and is, of course, exposed to the air. Bronze things eventually turn green if exposed to the air. So this statue will turn green.

Whoever established that the proportion of Swedes who are Catholics is zero, will have checked Petersen's religion, and thus in (iii) to check the backing is *ipso facto* to check the conclusion. In (iv) however, the backing for the warrant does not — indeed cannot — include the conclusion. Toulmin calls (iii) an analytic argument and (iv) a substantial one.

The discussion which leads to a definition of "analytic" is hard to follow. There are two different things that Toulmin might be doing here. He might be investigating the relations between an established concept of analyticity on the one hand, and verification, self-evidence and tautologousness on the other. Or he might be giving a stipulative definition of a new sense of "analytic", and pointing out that the distinction between analytic and substantial arguments to which it gives rise is not the same as a distinction based upon tautologousness or self-evidence. The definition at which he finally arrives makes it clear that he is introducing a new sense of "analytic", but he confuses the discussion by proceeding as if he were examining an established concept. He also confuses the grammatical sense of "tautology" with the logical sense of this word.

Let us consider an argument in which a modal qualifier is used.

(v) Petersen is a Swede. The proportion of Swedes who are Roman Catholics is five per cent. So it is almost certain that Petersen is not a Roman Catholic.

If we take the conclusion of this argument to be "it is almost certain that Petersen is not a Roman Catholic", then we have to explain how such a conclusion may be verified. This question does not arise for Toulmin, because for him "it is almost certain that

Petersen is not a Roman Catholic" is not a statement about the statement "Petersen is not a Roman Catholic". To say "it is almost certain that p" is to assert "p" in a guarded way. Whether this is right or wrong, it commits Toulmin to saying that the conclusion of (v) is "Petersen is not a Roman Catholic". The first consequence of this is that (v) is analytic since to check the backing would be to check the conclusion. The second consequence is that the premises of (v) may well be true and the conclusion false, and this despite the fact that (v) is by normal standards a sound argument. Toulmin accepts this and decides to "class an argument as analytic if and only if . . . checking the backing of the warrant involves *ipso facto* checking the truth or falsity of the conclusion . . . we shall do this" he says, "whether a knowledge of the full backing would in fact verify the conclusion or falsify it." (p. 133.) It follows from this that if we negate the conclusion of an analytic argument we obtain another analytic argument. Toulmin does actually say that analyticity, like formal validity, is not a criterion of soundness in an argument (p. 145), but sometimes he forgets how he has decided to use "analytic", as when he writes "In the analytic syllogism, the conclusion must in the nature of the case repeat in other words something already implicit in the data and backing; . . . " (p. 151).

(6) *Substantial Arguments and Type Jumps*

We have already noted the contention that the final justification for the step from C to D is the backing (B). The warrant (W) does justify the inference, but it in turn rests upon, and has no more factual content than, B. If the argument from D to C is a substantial one, then D and B does not entail C. Thus, if we hold that all sound arguments are sound analytic arguments (i.e. arguments in which the premises entail the conclusion), then we have to conclude that there is something wrong with the step from D to C. This is a mistake: there are good and bad analytic arguments and good and bad substantial arguments, but a substantial argument is not a bad argument just because it is not analytic.

The following are examples of the kinds of substantial argument in which Toulmin is interested:

(vi) He is smiling, so he must be happy.
(vii) This will cause a lot of pain and no pleasure, so it must be wrong.
(viii) The sky is clear, so there will be a frost.

In making each of these inferences, it is said, we jump from one logical type to another. To justify an argument from present to future (viii) we produce a warrant: often an empirical law. The law (W) is more than the observation statements (B) on which it is based. First, the step from B to W is often a very difficult one (cf. Kepler's Laws). Secondly, B may justify certain inferences, but, except in very simple cases, until W is formulated we do not know *what* inferences are justified by B. Thirdly, when a law is formulated, we do not jump from knowledge of some to knowledge of all, but, in addition to knowing that, for example, the planets were in such and such positions at such and such times, we do come to know that our observations satisfy certain formulae, and thus we come to know how to predict the positions of the planets. I think that Toulmin would agree with this, for he does say that although a warrant has no more factual content than its backing, it is not a mere repetition of the backing, but rather "a general moral of a practical character" (p. 106).

Although B & D & $\overline{C}$ is not a contradiction, I would suggest that W & D & $\overline{C}$ is. If facts can speak for a warrant, then they can speak against it, and D & $\overline{C}$ is just the sort of fact that does speak against W. Toulmin's point can still be made, but it now has the following more familiar form. Even though W is discredited, B is unscathed. Thus B does not entail W. If we believe that an argument cannot be sound unless it is analytic then we have to conclude that there is something wrong with the step from B to W. We may try to put this right, or we may not. In fact there is nothing to put right: there are good and bad deductive arguments, and good and bad inductive ones, but an inductive argument is not bad just because it is not deductive. Although Toulmin avoids the inductive-deductive dichotomy, it is not clear that this makes much difference to his analysis of arguments, for the advocated investigation of the field-dependent validity of substantial arguments does in the end come down to an examination of the way in which warrants are established in various fields. Nor is it clear that (vi) and (vii) give rise to the same problems as (viii).

(7) *Conclusion*

Because Toulmin repeats himself, he does not cover as much ground as one might expect, and this is unfortunate because his subject is of great interest and his treatment of it original. My main point against him is that he underestimates the importance of formal logic. In some places he takes for granted the ideas of logicians whose work he dismisses as irrelevant, while in others, when dealing with modal qualifiers for example, he makes mistakes that he could have avoided if he had paid more attention to work already done. The kind of analysis of arguments that is suggested in this book is a development in logic rather than a substitute for it.

In confining my remarks to what is said to be crucial in this book, I have omitted a great deal that is interesting and provocative. Indeed, if it is taken, as it is offered, as raising problems rather than solving them (p. 1), then *The Uses of Argument* will be a good starting point for the discussion of a large number of topics.

C F. PRESLEY

ETHICS AND THE MORAL LIFE. By Bernard Mayo. London, Macmillan, 1958. viii, 238 p. 34s. 9d. (Australian).

This book is a careful exposition and exploration of the view that moral principles are essentially "universal third-person imperatives" ("let everyone . . . ") : imperatives which are, paradoxically, both in the third person and addressed primarily to oneself.

Mr. Mayo is, as I should say, a subjectivist; or, as he would say, a relativist. He prefers to keep the term "subjectivism" for the view that moral judgments express one's likes and dislikes: that "I approve of capital punishment" is like "I like rice pudding". Objectivism, on the other hand, is the view that it is like: "Rice pudding is nutritious". The truth, Mayo tells us, is that it is like the one in some ways and like the other in other ways. Approving is different from liking because it commits one to advancing reasons for one's approval: and the reasons must take the form of principles which are "universalisable in three dimensions". That is to say, they must apply, not just to a particular action, but to all actions of the same class; they must, secondly, apply "not only to me but to you, not only to you but to me, not only to us but to everybody"; and, thirdly, "a moral judgment must be 'universalisable' in the sense that others besides the speaker are assumed to share it". (p. 91). It is this third feature that makes argument possible about morals, as it is not possible about tastes or feelings: when two people argue about whether an action is right, they are asking whether it does or does not follow from a principle which both are assumed to share. To this extent moral utterances resemble statements, since the criteria for empirical assertions are also assumed to be accepted by everyone; but objectivists are at fault, all the same, because they ignore the extent to which moral utterances function as imperatives

Mayo claims, then, to have settled the question at issue between subjectivists and objectivists, and to have steered cautiously between them. Both, he thinks, are mistaken. But he remains a relativist because, he says, the consensus implied by moral utterances does not actually exist. In disputes about matters of fact, there is always an implied appeal to the evidence of the senses; and we all do accept that, though of course we may differ about what that evidence points to. But in moral disputes it may sometimes happen that each party is implicitly appealing to a principle the other does not accept. In morals, then, there is a dissensus as well as a consensus. This is, Mayo tells us, no accident; he argues, indeed, that morality *implies* a dissensus just as it implies a consensus. To accept a moral rule is not only to commit oneself to a particular kind of action in a particular set of circumstances; it is also to join forces with others who accept the same rule and to "take sides" against anyone who either breaks the rule or challenges it. In making a moral judgment one is, of course, simply applying the criteria one *does* accept; and what is "right" by the standards of one "side" may well be "wrong" by the standards of the other.

It seems clear from this that the reasoned argument that is possible between those who accept the same ultimate moral principles will be very different from the disputes that will occur between those who do not. And this, I should have thought, is what subjectivists mean to assert when they say that moral utterances express emotions or desires: that when we come to consider these ultimate moral principles we find that we can only accept or reject them, much as our palate accepts or rejects rice pudding. And this does seem to be Mayo's position, too. His distinction between what we like and what we approve seems identical with Russell's distinction between personal desires (like a taste for rice pudding) and impersonal desires (like a preference for a world in which there is no capital punishment). The point about impersonal desires, according to Russell, is that we want others to have them too: and it is these desires that moral utterances express. A subjectivism of this type would seem to allow sufficiently for what Mayo says about the "three dimensions of universality". Of course, there is no reason why Mayo should not use the term "relativism" for a position of this kind, and reserve "subjectivism" for a cruder (but surely seldom held?) view which refuses to recognise *any* difference between personal and impersonal desires. But he should not assume that, in conceding the strength of the objectivist case against "subjectivism", he he has said all that he need say about their case against "relativism". It may, indeed, be possible to answer the objectivist arguments; but I doubt if Mayo has done so.

Having put the case for relativism in Part One ("Moral Discourse") Mayo goes on, in Part Two ("The Moral Life") to elaborate the ethics of commitment. Two of the four chapters in this Part are concerned with the "authority" of conscience. In using this term, Mayo points out, we are using a political analogy; and he goes on to discuss the various ways in which both persons and rules may be said to have authority. His conclusion is that: " 'Conscience' is not the name of any faculty or psychical component. Its function is to figure in expressions which refer to the issuing of self-directed commands in accordance with moral rules which we accept as authoritative." (p. 171). Moreover, "what is peculiar about moral authority is not that it is internal but that it is, in an important sense, unchallengeable. All other sources of authority can be challenged . . . One can, by suitable acts of defiance, reject the authority of the State, the Church, the M.C.C. or any other organisation . . . But in practice one usually rejects a certain authority for what one regards as good reasons; and the repudiation takes the form of a preference for some other authority. One may reject the authority of one's schoolmistress who forbids make-up in favour of one's parent who permits it; one may reject the authority of the state which orders training in the art of warfare in favour of one's pacifist convictions . . . Either all our various sources of authority are recognised only provisionally, that is, we regard none of them as beyond challenge; or else there must be some one source of authority which we recognise as unchallengeable, and by reference to which all other authorities can be challenged. This second possibility must be the case if there really are moral principles, as distinct from other kinds of principle. We regard man as a moral agent, only so long as we regard him as having the right to set himself up against all external authorities whatever, and to make up his own mind what he ought to do, irrespective of what he may be commanded to do" (pp. 169-170).

What this amounts to, I think, is that a moral consideration is (by definition) whatever consideration "weighs most" with an individual. There must be some reasons which we regard as final and these must be self-commands. Even if a man feels that he ought not to set up his own conscience against the authority of the Church, the State, or the M.C.C., he is accepting as final the rule that "one ought not to set up one's own conscience, etc." So that there is at least one self-command which he takes to be authoritative, even if it is merely the command to do whatever someone else commands. Whatever considerations an individual accepts as final, these constitute his morality.

In Part Three ("Towards a New Humanism") Mayo suggests that the ethics of rules and principles constitute only a part of the moral life. There is the ethics of "goodness" as well as of "duty": of "being" as well as of "doing". As well as the question "What shall I do?" to which the answer is: "Observe such-and-such a principle", there is the question "What shall I be?", to which the answer is: "Be like so-and-so", where so-and-so is either an actual man (Socrates, St. Francis, Napoleon) or an ideal type (Plato's "just man", "the good communist", "the hundred percent American"). Mayo has two main points here, I think One is the familiar one that the system of principles one accepts as final constitute "a way of life". The other is that such a way of life is not fully described when it is thought of as a system of principles: it is rather the expression of a set of dispositions, or a "character".

In the book as a whole Mayo is obviously putting forward an ethical position that is now familiar and that has caused (and deserves) a good deal of discussion: discussion to which this book makes a real contribution One cannot consider this position fully in a review; but Mayo's presentation of it suggests two comments.

(1) It is not clear that the "dissensus" he talks about is bound up with the meaning of "morality". Certainly moral principles presuppose conflict, including conflict within oneself. No doubt to accept a moral rule is to "side against" those who break the rule, including oneself when one breaks it. But to break a rule, as Mayo himself points out, is not to challenge it. Moral principles do presuppose the possibility of their being broken; but not necessarily of their being challenged. It may

well be a fact that any given moral principle is rejected by someone; but it does not follow from the nature of moral principles that this must be the case.

(2) In effect, Mayo advances two different definitions of "moral", which may conflict. In Part One we are told that moral principles are those which are universalisable in the three "dimensions" he specifies; in Part Two that moral principles are those which any given individual takes as final. Now each of these corresponds to an accepted use of the terms "moral" and "morality". We may say of a selfish man, for example, either: "With him selfish considerations override moral ones"; or, "His morality is a purely selfish one". But we cannot conclude, as I think Mayo and others of this school do, that one ought never to accept as final principles that are not universalisable. We may well want to assert this and it may well be true; but we cannot regard it as following from the very meaning of moral terms. To think that we can is to confuse these two uses of "moral". The point is even clearer if it is put in terms of Russell's "personal" and "impersonal" desires. We may, if we like (and with considerable support from usage) define "moral principles" as "the expression of impersonal desires"; but we cannot conclude from this that we ought always to prefer impersonal desires to personal ones.

D. H. MONRO.

AN INTRODUCTION TO THE PHILOSOPHY OF EDUCATION. By D. J. O'Connor. London: Routledge & Kegan Paul, 1957. 148 p. 10s. 6d. (U.K.).

Too many philosophers nowadays treat education and its problems as if they were merely of secondary importance. Academic teachers are, on the whole, prone to be oblivious to the fact that they are also pedagogues. And thus it is only natural that they do not consider the philosophy of education as being worthy of a rigorous inquiry.

Professor O'Connor, though first and foremost a logician, has dealt with the philosophy of education from the point of view of a philosophic analyst. I think he has done future teachers a great service by his brief and lucid definition of the function of philosophic investigation in general. No doubt, to the uninitiated his unromantic, unmetaphysical treatment will come as a shock. However, there is not the slightest evasion of central issues throughout the whole discussion. On the contrary, it is one of the great virtues of O'Connor's book that it challenges established common notions about the aims of education.

What is the goal of all educationalists? How can it be most effectively attained? In order to answer these two vital questions the reader is introduced to a close scrutiny of value judgments, of the meaning of 'theory', of scientific methodology. Some formulations may antagonize even a reader who concurs, on the whole, with O'Connor's general outlook. "We have the same reason for rejecting metaphysics as we have for rejecting witchcraft, astrology or phrenology; it cannot do what it claims to do" (p. 114). And not every votary of logical empiricism will submit to the dogmatic statement that the outcome of philosophizing "has the negative effect of an intellectual antiseptic . . . " (p. 113). On the other hand, one need not be a militant atheist to accept his claim that we have no reason to believe "that there is a connexion of a logical kind between the rules of morality and certain theological statements such that the former are entailed by the latter" (p. 128).

The author regards it as one of the primary ends of education to make man critical. Such questions as vocational training and the awakening of the student's desire for knowledge are adequately appraised. It is also obvious that O'Connor relegates the most essential activities of educational practice to psychology, sociology and the biological sciences. He does not attempt to develop an autonomous educational philosophy. Rather he insists that the true problems of educational theory are universal philosophic problems. For instance, an educator ought to realize that moral judgments are not genuine statements, but mere expressions of approval or disapproval. A philosophy of education has to inquire into the nature of truth, certainly, the concept of mind, the objective criteria for propositions and so forth, no less than any theory of knowledge proper.

As is to be expected, O'Connor deplores the fact that metaphysical doctrines have exerted their dangerous influence not only upon the aims, but also upon the methods, of education. Even those of us who are not avowed followers of fashionable linguistic analysis will agree with the author that a careful study of the relations between the expressive, informative and persuasive uses of language is a necessary prerequisite before we try to find out how value judgments can be justified

The professional educationalist will, to be sure, vehemently reject O'Connor's lapidary assertion that "the problems of the philosophy of education, if pressed far enough, become the traditional problems of philosophy" (p. 140) ; or that, as stated above, there is only philosophy, but no philosophy of education. I for one do not fully share O'Connor's final conclusion. Yet I have to admit that there is hardly a scintilla of evidence for a refutation of his argument.

All in all, this provocative and often incontrovertibly convincing essay is an incisive piece of work and, though not always cautious, inexorable in its findings. The significance of this book is a fact which this reviewer is only too glad to acknowledge. It is a commonplace that criticism can desiccate. O'Connor's approach brings fresh waters to the roots of educational theory.

WOLFGANG YOURGRAU.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

BRAIN, Russell. Science, philosophy and religion; the twelfth Arthur Stanley Eddington Memorial Lecture, 24 February 1959. Cambridge University Press, 1959. iv, 31 p. 4s. 6d. (U.K.) Paper covers.

BRIGGS, Michael H. Handbook of philosophy. New York, Philosophical Library, 1959. 214 p. $4.75.

Brief definitions of philosophical terms for the beginning student

DEWEY, John. Moral principles in education New York, Philosophical Library, 1959. xi, 61 p. $2.75

A reprint.

GIBSON, John M. Soldiers of the word: the story of the American Bible Society. New York, Philosophical Library, 1958 304 p. $3.75.

GREGORY, C. C L. and KOHSEN, Anita. The O-structure; an introduction to psychophysical cosmology Church Crookham, Institute for the Study of Mental Images, 1959 141 p. 21s (U.K.)

An attempt to construct a world-model which will allow for recent developments in physics, biology, parapsychology, and information theory

HARRIS, Errol E Revelation through reason; reason in the light of science and philosophy. London, Allen and Unwin, 1959. 123 p 15s (U.K.)

"Religion and science (are) not really two different departments of human experience, but . . . one and the same thing. Moreover, consideration of the trends observable in modern scientific thought leads to a view of the world which, so it appears to me, necessitates belief not only in God but in such a God as would justify the main doctrine of Christianity. that Jesus is the revelation in human form of the divine." Preface Originally Terry lectures at Yale

HALMOS, Paul and ILIFFE, Alan eds. Readings in general psychology London, Routledge & Kegan Paul, 1959 ix, 251 p 25s. (U.K)

HAWLEY, Don The nature of things New York, Philosophical Library, 1959. 187 p. $3.75.

"A logical destruction of the concept of materialism . . . a bold challenge against . . . spiritual pessimism." Blurb Argues, for example, that the atom bomb was foretold in the Book of Revelations.

HEISENBERG, Werner. Physics and philosophy, the revolution in modern science London, Allen and Unwin, 1959. 176 p 15s. (U.K.)

Discusses the philosophical implications of quantum theory.

HEYWOOD, Jean S. Children in care; the development of the service for the deprived child. London, Routledge & Kegan Paul, 1959 x, 229 p. 25s. (U K)

HEYWOOD, Rosalind. The sixth sense; an inquiry into extra-sensory perception. London, Chatto & Windus, 1959. 224 p. 21s. (U.K.)

Describes and discusses the principal experimental evidence for ESP.

HIRST, R. J. The problems of perception. London, George Allen and Unwin Ltd., 1959. x, 330 p. 30s. (U.K.)

Examines critically various theories of perception and offers an explanatory hypothesis, described by the author as "a version of monism which may be classed as a Double Aspect Theory . an Identity Hypothesis".

HOOK, Sidney, ed. Psychoanalysis, scientific method and philosophy; a symposium. New York University Press, 1959. xiii, 370 p. $5.

The proceedings of the second annual New York University Institute of Philosophy, 1958, attended by philosophers, psychologists, psychoanalysts and sociologists. The main discussion is about the scientific status of psychoanalysis, with papers by Nagel, Hartmann and Kubie, and comments by Arlow, Hook, Pap, Hospers and others. There is also a psychoanalytic "explanation" of philosophy, by Laserowitz, with an angry retort by Donald Williams, and a rather more effective one by Flew; and papers on the contribution of psychoanalysis to the social sciences, by Kardiner, van den Haag, and Inkeles.

HULL, L. W. H. History and philosophy of science; an introduction. London, Longmans, Green, 1959. xi, 340 p. 43s. 6d. (U.K.)

Mainly history; but "the theme is the relation of science to thought rather than action" (Preface) Hence there is some discussion of Plato, Aristotle, Locke, Berkeley and Hume.

KRIKORIAN, Yervant H. and EDEL, Abraham. Contemporary philosophic problems; selected readings. New York, Macmillan Co., 1959 xi, 712 p. $7.

There are five sections, on "Meaning", "Knowledge", "Being", "Value", and "Vision and Action". A mixed lot of authors, including (e.g.) Gandhi, Heidegger, Spengler and Niebuhr as well as Peirce, Broad, Ayer, Russell, Bosanquet and Ryle.

LAMONT, Corliss. The illusion of immortality. 3rd ed. New York, Philosophical Library, 1959. xv, 303 p. $3.95.

First published 1934, but now revised and rewritten Presents the case against continued existence after death.

LAZOWICK, Frank E. The science of philosophy. New York, Philosophical Library, 1959. ix, 379 p. $5.

"The seven Dimensions of dynamic Process and of Being characterize all objects", viz., Faith, Wisdom, Justice, Might, Freedom, Love and Beauty.

MARX, Karl. A world without Jews; edited, with an introduction, by Dagobert D. Runes. New York, Philosophical Library, 1959 xii, 51 p $2.75.

Claims to be "the first unexpurgated English language publication" of two short papers, "The Jewish Question" and "The Capacity of Today's Jews and Christians to Become Free", published by Marx in 1843 in reply to Dr. Bruno Bauer.

MAYS, W. The philosophy of Whitehead (Muirhead Library of Philosophy.) London, Allen & Unwin, 1959. 259 p 25s. (U.K.)

A commentary on, and re-interpretation of, *Process and Reality*. Argues that "the key to the understanding of Whitehead is to be found in his earlier philosophy of science".

MILES, T. R. Religion and the scientific outlook. London, Allen and Unwin, 1959. 224 p. 21s (U.K.)

Argues that any conflict between science and religion can be avoided by regarding religious language as "the language of parable", which makes no factual assertions about the nature of the universe. "To accept the theistic parable is to commit ourselves to a particular way of life."

MORTIMER, Ernest. Blaise Pascal; the life and work of a realist London, Methuen, 1959 240 p. 21s (U.K.)

ORTEGA Y GASSET, José. Man and crisis; translated by Mildred Adams. London, Allen and Unwin. 1959 217 p. 16s. (U.K.)

A study of ages of transition, such as the Renaissance, or our own time

PEIRCE, C. S. Collected papers of Charles Sanders Peirce, ed. by Arthur W Burks: volume vii, Science and philosophy; volume viii, Reviews, correspondence and bibliography. Cambridge, Mass, Harvard University Press, Oxford University Press, 1958 xiv, 415 p + xii, 352 p. £4 4s. ea. (Australian).

"Many of the manuscripts have never been published before, and the previously published material . is widely scattered in a number of journals" Blurb.

SCHWEITZER, Albert. The light within us. New York, Philosophical Library, 1959. 58 p. $2.75

Brief aphorisms selected from Schweitzer's writings.

STRAWSON, P. F. Individuals; an essay in descriptive metaphysics London, Methuen, 1959. 255 p. 25s. (U.K)

"The first part aims at establishing the central position which material bodies and persons occupy among particulars in general . . In the second part of the book the aim is to establish and explain the connexion between the idea of a particular in general and that of an object of reference or logical subject." Introduction.

TOMKINS, Silvan S. and MINER, John B. PAT interpretation scope and technique. New York, Springer, 1959. 184 p. $5.50

A "picture arrangement test" for assessing personality

WEITZ, Morris, ed Problems in aesthetics, an introductory book of readings New York, Macmillan Co, 1959 ix, 697 p. $6.90.

"I have tried to present the best discussions of the main problems of æsthetics regardless of their dates of composition . . I have organized the readings around certain sets of basic problems." Preface.

ZUURDEEG, Willem F. An analytical philosophy of religion. London, Allen and Unwin, 1959. 320 p. 30s. (U.K.)

Claims to be an analysis of religious language which attempts to be "as unbiased as humanly possible", and hence not, like other recent works, "a defence (or attack) of the Christian claim in terms of the new philosophy".

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HARE'S ETHICAL SUBJECTIVISM

By H. J. McCLOSKEY

In this paper I shall be concerned to consider how the main traditional objections to subjectivism tell against one of the more notable of the various contemporary varieties of ethical subjectivism, namely that developed by R. M. Hare in *The Language of Morals*.[1] At the same time, I shall seek to show how attempts to circumvent these traditional objections not only fail, but also lead to new, equally serious difficulties.

Traditionally the main criticisms of subjectivism relate to the apparent inability of a subjectivist to give adequate accounts of the justification of moral judgements and moral principles; of disagreements in morals; of the truth or falsity of moral judgements and principles; of personal deliberation and perplexity and inter-personal guidance advice, instruction and exhortation; and of the reality, authority, and autonomy of morality. Subjectivist theories seem to imply that ethical judgements and principles cannot really be given an ultimate rational justification; that the most typically moral disagreements are not real clashes of logically contradictory propositions; that ethical judgements cannot properly be said to be true or false — unless they are explained as autobiographical reports; that personal deliberation and perplexity, and inter-personal advice, are not what they appear to be; and that there are no rational grounds, or at the most comparatively weak grounds, for attributing to morality the authority we commonly attribute to it. This account of morality, which detracts so much from the authority of morality, seems at the same time to reveal the ethical subjectivist — who in other spheres is often modest and even humble — as a man of great arrogance, in persisting in trying to impose his 'moral judgements' on others, after he, as a subjectivist, has grasped their true import. The subjectivist analysis, again, is commonly criticised because it fails to provide an adequate basis in terms of which the moral and the non-moral, for instance morals, manners, tastes, etc., may be discriminated. And it makes puzzling the use of such expressions in ordinary discourse as 'knowing', 'believing', 'judging', 'seeing', 'recognizing' something or other to be good, or to be obligatory. So too, immorality, its nature and its possibility, may create difficulties for some varieties of subjectivism, as may the specifically moral feelings of moral indignation, guilt and remorse.

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[1] The term 'ethical subjectivist' is here used in the conventional sense to refer to theories of the type which make moral expressions basically dependent on the thoughts, feelings, attitudes or wishes of moral agents, and which accordingly proceed to explain moral judgements as disguised statements or expressions of feelings, exclamations, or as commands or imperatives or the like, and of which the theories of Carnap, Ayer, Stevenson, Hare and Nowell-Smith are instances. Although Hare, and apparently also Nowell-Smith, decline the title, there is no real impropriety in describing them in this way.

An acute exposition of most of the traditional objections referred to here is given by A C Ewing in *The Definition of Good*, Ch. 1

The fundamental difficulties seem to be those relating to the possibility of rational justification of moral judgements and principles, to the nature of moral disagreements, and to personal perplexity and inter-personal advice. The other difficulties either follow from these or arise from the same features of subjectivist analyses as give rise to these basic difficulties. It is to these difficulties that first Stevenson, then Hare, and more recently Nowell-Smith, have directed much of their attention; but in spite of their various modifications and qualifications of their subjectivist themes, they do not appear to have been successful in satisfactorily meeting them This is now generally conceded in respect of Stevenson's variety of ethical subjectivism. Let us now, then, consider whether it is true, as has been claimed, that these traditional objections to ethical subjectivism are fatal to Hare's theory.

Hare, in introducing his book, *The Language of Morals,* explains that it has arisen out of his dissatisfaction with previous subjectivist theories — particularly that of Stevenson — as accounts of the rationality of morals. However, this introductory explanation builds up expectations not subsequently fulfilled. Hare does not set out a clear and detailed theory; nor does he offer a more convincing account of the rationality of morality than does Stevenson. In brief, Hare gives the impression of providing material from which an interesting, if more ingenious than plausible, moral theory may be constructed, rather than of having actually constructed a finished product. He spends a lot of time explaining his prescriptive theory of the logic of moral terms in terms of non-moral prescriptions. And towards the end of the book he seems to mean to assert much less than at the outset when he claims that moral expressions are primarily self-embracing, universal prescriptions. His book gives the air of great logical rigour; yet when we extract and examine the theory it contains, we find that it provides no stronger a basis for ethical subjectivism than does Stevenson's analysis—rather, if anything, a weaker basis. And the one contribution of value, that concerning the universality of moral principles, is taken from Kant and incompletely worked out and made to do too much by Hare, but later used with more understanding and insight by Nowell-Smith.[3]

Hare's basic distinction is that between prescription and description. This springs from his concentration on what it is now fashionable to call 'ought'-sentences; and this concentration in turn springs from his view of morality as essentially concerned with action. Moral judgements are thus represented as a special kind of answer to questions of the form: 'What shall I do?'. All answers to such questions are said necessarily to be commands, and, in the case of moral answers, they are said to be commands derived from universal, self-inclusive, self-committing prescriptions.

Hare says comparatively little about the logic of these universal prescriptions. His account of the logic of prescriptive language centres largely around imperatives, the non-normative species of the genus prescriptive language. The key to his discussion here is his distinction between the ***neustic*** and ***phrastic*** elements in the meaning of imperatives, and the contention that imperatives have a logic like that of indicatives, and hence admit of rational support.

If we follow the clues given in the analysis of the imperative, 'Shut the door', it would seem that the principle of utility should be analysed as: 'Everyone, past, future and present, is happy, please' And the principle relating to cruelty to animals would be elucidated as: 'Everyone, past, present and future, will refrain from cruelty to animals, please.' (The word 'please' is what Hare calls the 'neustic' element, and the rest of the analysis is what he calls the 'phrastic' element of these universal prescriptions.)

This may look as if it is a parody of Hare's theory. It is not. These analyses are what follow most obviously from Hare's analyses of imperatives. In any case, that this is the correct interpretation becomes more evident when the rest of Hare's account of morality is considered.

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[3] In spite of its greater subtlety and insight, Nowell-Smith's discussion of the distinguishing marks of a moral principle is by no means satisfactory It is not possible to show this here; and, in any case, it is sufficient for our present purpose to observe Nowell-Smith's awareness of the need for a more subtle discussion than that which appears to have satisfied Hare

Morality is explained in terms of decisions. A moral decision about a particular matter, culminating in the statement, 'I ought', or 'You ought', represents the adoption of a universal principle. And the decision of principle is the formulation of a prescription for all others, at the same time as committing oneself. Thus if the imperative model is at all illuminating, as Hare presumably thinks it to be since he devotes so much space to it, then it follows that these self-committing universal prescriptions, which underlie or comprise the whole of a moral decision, must be analysed more or less along the lines of the example given, 'Shut the door'. Thus, to take Hare's example — No Smoking in Railway Carriages — his model of an analysis of a moral prescription would seem to be: 'No one is ever going to smoke in any railway compartment anywhere, please'. This is a universal imperative which Hare thinks no one would ever have occasion to lay down; but it would seem to be one which, in terms of his analysis, would become a moral principle, if it were laid down as a self-committing universal prescription.

The point of the analysis of imperatives is to reveal the logical form of prescriptions and to show thereby how and in what ways imperatives and prescriptions generally are subject to logical rules, may enter into inferences, admit of rational support, and genuinely contradict one another.

If imperatives are governed by logical rules, if they may be genuinely contradictory, and if they may be supported by reasons, some of the sting is taken out of the traditional objections that subjectivism does not account for and explain the apparent genuineness of moral disagreements, and that it does not explain the possibility of rational argument in morals. However, the whole force of these general objections is not lost, unless it is shown that decisions of an ultimate character are not mere capricious, arbitrary decisions, nor even decisions of a non-rational character, i.e., that moral reasoning at all levels can be explained on the model of reasons relevant to imperatives.

It is here that we come to the crux of Hare's theory. Decisions of an ultimate character, once all the facts have been accumulated, are said simply to be up to the person — whatever that could conceivably mean. The individual is said to be moral just so long as he makes this moral-type, self-committing, universal prescription and then obeys it; or alternatively, apparently, he is not immoral, provided he does not make a decision of this character, and hence is not in a position to disobey anything.

Which decision, which universal principle of action the individual makes, may affect us, and be such that we cannot remain indifferent to it, because we have to live in the same world; but we cannot say that it is wrong or incorrect, nor even that it is immoral, except as a means of recommending, in a roundabout way, our own universal prescriptions.

It is this apparent moral indifference to the question of which decision of principle a man reaches which has led to Hare's being linked with existentialists such as Sartre, and this, in turn, has led to the condemnation of his theory as being therefore unsuccessful as a demonstration of the rationality of morality in terms of a subjectivist analysis. However, Hare's analysis does not bring him quite as close to existentialist ethics as is commonly supposed. Hare himself does seem to believe that it is better to make these decisions of universal principle than not to make them, but this is not an implication of his analysis. Hare's analysis implies simply that you enter the sphere of morality by making decisions of universal principles, that you are non-moral, or outside the sphere of morality, in so far as you do not make such universally prescriptive decisions, and that you are immoral only in so far as you disobey your self-imposed, universal prescriptions. It carries no implication that we ought to make such decisions.

However there is, none the less, point in drawing the parallel with existentialism in that, on Hare's analysis, morality, for the individual deciding on a course of conduct, consists not in adopting the right or moral way of life — there is and can be no such thing, according to Hare, before the decision, if the decision relates to ultimates — but simply in self-committal to one of these universal prescriptions. Provided he has committed himself, the individual is right and cannot ever be shown or ever himself discover that he was wrong; and his conscience can remain completely clear, On this view there can be no personal wickedness in choosing the wrong ultimate principles, and no personal immorality in not adopting a principle, such as the principle of utility or truth-telling or abstinence from cruelty; nor even in not adopting a universal principle of action in, say, a whole area of our life, where others might think it essential — e.g. in our treatment of animals, or in sexual behaviour. We may of course find it difficult not to adopt universal principles — Hare seems to think that quite a number are unavoidable — and we may meet with condemnation from others for various of our decisions; and we ourselves may come to adopt other ultimate principles; but if we at the time of our decisions are sincere in our self-committal to a universal prescription, we cannot, in terms of Hare's analysis, fear that we may have adopted an immoral principle. And when we are hovering between two alternative prescriptions to lay down for ourselves — of altruism or egoism — *we cannot be thought of as wavering between the moral and the immoral, for neither becomes the moral or the immoral for us, on Hare's analysis, until the decision has been made.* And then immorality would seem to consist in failing to live up to the principles to which we have committed ourselves, and prescribed for others. These implications and aspects of Hare's analysis emerge very clearly from the discussion of ultimates on pages 69-70 of *The Language of Morals.*

Critical Appraisal:

Let us now consider the soundness of this theory, and in particular whether, in terms of it, Hare is able to make good his claims to have made a significant advance on previous subjectivist theories. As has been noted, the difficulties of these earlier theories — Stevenson's and its predecessors — relate on the one hand to the problems of marking off the moral from the non-moral, and on the other to the claims of moral judgements and discourse to be regarded as rational, as true or false, as possessed of authority, and as being generically different from discourse concerning tastes, customs or law. A brief consideration of Hare's analysis, in the light of these difficulties, makes it immediately apparent that Hare's analysis is a very questionable advance on its predecessors. It succeeds in meeting some traditional difficulties only by becoming more seriously exposed to other traditional objections, and by encountering in addition special new difficulties peculiar to itself. To consider first the three main objections that relate peculiarly to Hare's theory, and then how the main traditional objections apply:

1. It is *prima facie* unplausible that moral judgements and principles are prescriptions. By the end of his book, Hare waters down what he means to assert by this claim, but it remains true that moral judgements such as 'It is wrong not to repay your debt to Smith' and moral principles such as 'It is right and obligatory that wives should obey their husbands' are more like statements than prescriptions, although in certain respects distinct from both. Stevenson's analysis had the very real merit of being a product of an attempt to take note of both features of moral expressions. Hare, on the other hand, seems just arbitrarily to reject the propositional characteristics of

ethical judgements as misleading, or as adequately accounted for in terms of the phrastic element of imperatives. Yet moral expressions are much more like statements than are imperatives.

This prescriptive analysis is completely inappropriate and irrelevant to ethics. But, even if it were not inappropriate, Hare's account of the logical structure of imperatives in particular, and of prescriptions in general, would have to be rejected as unsatisfactory. Quite apart from making it appear that, when we think imperatives in terms of their logical form, we think as some nervous, halting foreigners speak — You are going to shut the door, please — (and this is ludicrous enough even in a long tradition of ludicrous logical reductions and analyses), it is unsatisfactory because incomplete and non-explanatory. The account of the logical function of 'please' is needed to complete the explanation, but this is nowhere given.

Again, if this sort of analysis were to be accepted, it would need at least some sort of development of the kind suggested by Braithwaite, in terms of distinguishing many more neustics than simply 'yes' and 'please'. Braithwaite suggests that this sort of modification would lead to an account of moral expressions as resolutions and subscriptions, rather than as prescriptions [3]

Thus Hare's analysis of imperatives is defective because too crude, and because incomplete, and, more importantly, because it is inappropriate or irrelevant to ethics. The importance of this criticism lies in the fact that Hare's analysis of imperatives is basic to his claim to be able to show morality to be rational.

2 It is commonly argued that Hare's theory is both fundamentally inconsistent and incomplete. Hare bases his prescriptive analysis on three claims — (*a*) that moral judgements are answers to the question, What shall I do?; (*b*) that the answer to this question must be a command; and (*c*) that prescriptions can be deduced only from prescriptions. Yet, in the celebrated passage on decisions of principle, as well as in many other places — e.g. in his discussion of changing societies, and in his 'sermon' on educating one's children — he talks about these universal prescriptions as being ***based upon*** certain facts, and as being ***suitable*** or ***unsuitable*** to certain facts. That is to say, Hare, in defending his theory, moves from 'is' to 'ought', or, in his terminology, from indicative to prescriptive. This is not an accidental lapse. but one made necessary by his attempt to explain ultimate decisions

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[3] Critical Notice Hare—*Language of Morals* Mind. 1954

Lurking behind much of Hare's talk — talk about ***basing*** prescriptions on the facts, of *verifying* our decisions of principle, and of reaching ***important*** conclusions in decisions of principle, and the like — is a theory about *the reason* for these universal prescriptions. It is, I suspect, a theory about natural pro-attitudes, for that would explain why Hare is so confident we will all become involved in the game of morality; but the theory, whatever it be, is not made explicit, and, until it is, the theory, whilst being directed at explaining the logic of moral expressions, is seriously incomplete as an account of the purpose and use of moral expression.

3. Hare's whole analysis proceeds from the view that moral principles are special types of decisions of principle. However, if we compare a paradigm case of a decision of principle with moral deliberation culminating in the acceptance of a moral principle such as the primary precept of the natural law, we find that there is a very considerable difference. A decision of principle might be that of an inaccurate full-forward deciding always to look at the ball and not at the goal when kicking for goal. We can imagine how he might come to adopt this principle — that he is distracted by the crowd and by the man on his mark, made uncertain by being over-conscious of aiming at goal, etc. This is not at all like suddenly discovering that the Angelic Doctor was right after all, that we should: Follow Nature!

To consider now how Hare's account fares in respect to the more general, traditional objections to ethical subjectivism:

4. Clashes of ultimates are clashes within morality, such that which resolution is arrived at is not morally indifferent. They are moral clashes. To explain: Recently I had reported to me a conversation between two apparent murderers, the conversation occurring in a bar known to be patronised by Melbourne's underworld. One man expressed dislike of killing men with wives and children, and even had some scruples about it. The other man brushed these scruples and feelings of distaste aside, observing that if such killing was for money then it was all right, whether the victim had dependants or not. Consider a conflict between these men, and between each of them and an ordinary person. Should we consider that it was morally indifferent how such a three-cornered disagreement was settled? My point here becomes clearer if we consider a fourth person, in doubt about the rights and wrongs of the conflicting attitudes. He wants to reach the *right* decision, and not simply to be brought to *a* decision.

Clashes about ultimates are moral clashes. As our example brings out, this is most apparent when we consider genuine personal moral deliberation or perplexity. The individual who is genuinely perplexed about moral ultimates — as an onlooker might be in the example just cited, or as so many are in modern society, when faced by the clash of justice and happiness on the one hand against advancing culture on the other — wishes to reach the morally right conclusion. Hare fails to explain such facts; and in failing at this point his theory fails to explain moral disagreements at the point at which they are most characteristically moral.

On Hare's view, there is no morally right or wrong principle for the perplexed person to discover. The principle only becomes right by being adopted and prescribed. Similarly, whilst the objectivist must admit that there may be clashes concerning ultimates such that no further reasoning becomes possible, there is none the less a fundamental difference between his position and that of Hare, and, on this issue, his would seem to be the characteristically moral position. When a disagreement concerns ultimates, and is such that further reasoning is impossible, the resolution of the disagreement does not suddenly become morally indifferent, and certainly not just on this account. It is not a matter of individual whim which decision is to be reached. The disagreement remains a moral one in the sense that which solution is reached is not morally indifferent. Hare, on the other hand, explains it as one which is morally indifferent, and implies that a decision or resolution is equally moral and equally reasonable, whichever way it goes. And it would seem to follow from his analysis that if we reach such ultimate conflicts, and are in full possession of the facts, it is a morally sound way of overcoming our perplexity to toss a coin, or, if we can avoid a decision of universal principle, then perhaps to do that instead Clearly, in arguing in this way, Hare, in terms of his analysis, takes away all reason for attributing *authority* to morality, the more so as we are said to be the creators of our own universal principles.

It would seem therefore that Hare fails to explain the rationality of morality at the most important points in morality. In failing in this, he at the same time fails to explain satisfactorily the authority of morality, the nature of and reasons for moral disagreement, and our reasons for persisting in such disagreements.

To explain these latter points. Clashes of ultimates are said to be clashes of universal prescriptions, which cannot both be obeyed. But why are these contradictory imperatives advanced and persevered with in the light of disagreement? In fact, why are they advanced at all? No theory is complete which fails to answer these questions; yet Hare does not do much by way of attempting to answer them.

Hare's account of why we coerce others — i.e. of why we persist with our moral prescriptions in the face of opposition — is in terms of our having to live with those with whom we disagree. But this is hardly satisfactory. Why should we not bring ourselves into line with others? And why do we not simply agree to differ, as we do in other matters where we do not think uniformity is essential? In sexual morality, and in many other areas in morals, considerable diversity of opinion and practice prevails in the community without serious disharmony or disruption to social life.

The truth of the matter is that Hare's explanation presupposes an independent judgement of importance outside of morality. We persist with our moral disagreements, and not with, or so strongly with, those concerning tastes, manners or conventions, because moral disagreements and moral matters are important and the others less important: and the importance is not to be explained completely in terms of our just feeling strongly about our own prescriptions, for, if that were the whole story, Hare's account might make it rational for us to abandon some or all of our prescriptions to achieve greater harmony. But moral prescriptions are not like that They are not things we can accept or reject at will, for they seem not be wholly matters of choice, or creations of our own, as Hare's account suggests, but rather they are things forced upon us by the facts.

In the same way, Hare's account gives no grounds for individuals entering into the sphere of morality, i.e., for making these self-committing, universally prescribing decisions. His theory creates a problem which does not arise at the level of ordinary moral expressions, the problem namely of explaining why we should enter into the sphere of morality by using moral discourse. The point of moral expressions is that they seem to carry their own explanation or justification. Moral talk explains why it is engaged in and why it ought to be engaged in. Hare's analysis does not bring this out. It does not reveal why we should not withdraw from morality where we can do so, nor why in practice it is virtually impossible to do so. Why should we commit ourselves and others to general policies? Why shouldn't we just legislate for ourselves, and only for ourselves, where possible? And why shouldn't we simply live piecemeal as far as principles of action are concerned? Why should we be so keen to commit ourselves in the future?

Hare could attempt to meet these difficulties in various ways, but each way would itself lead to very serious difficulties in its turn. In any case, he would still have to account for the large areas of our lives left unprescribed, and he would also have to explain why some people are moral with a lot of lesser principles, and others are equally moral with big ways-of-life principles. Further, even though it may be a feature of moral language that it is a statement of personal commitment — and even this is arguable — the question still arises as to why someone should not opt himself out of his commitment. That is to say, why should he not confine himself to prescriptions for others and not for himself? Hare seems to want to say here that such a person would be contradicting himself. But why should he not use language arbitrarily, and why should he not contradict himself, if it suits him? Hare's account of the logic of moral terms seems to provide no basis for condemning such a person, except in the concrete situation, as one who does not accept our prescriptions.

Thus, in brief, Hare's analysis encounters serious difficulties in explaining the rationality of morality at the most important points, viz., the authority of morality. the nature of and reasons for moral disagreement, and for persistence in moral disagreement, and, more generally, why the individual should involve himself in moral commitments

5. Hare seeks to discriminate moral prescriptions from non-moral prescriptions by reference to moral prescriptions as universal prescriptions, self-imposed and self-prescribed. This formula is completely unsatisfactory. Not all moral principles are universal and self-committing in the same senses, and not all self-committing universal prescriptions are moral principles.

To explain first concerning their alleged universality. 'Universal' could mean 'applying to all men', or simply to classes of men. If it means 'applying to all men', and only this, it would mean that this formula excludes a good deal normally thought to be within the sphere of morality, as well as including many principles not normally considered to be moral principles. The principle relating to truth-telling is completely universal — it relates to all, both as subjects and as objects. The principles of respect for one's elders and the dead bind all rational beings but relate only to a specified section of mankind. The principles relating to promise-keeping and repayment of debts apply only to those who enter into debts and promises — and this need not be all men — and they relate only to those who enter into promises and debts. The principles relating to punishment, stealing and adultery have a more restricted universality in the sense that they presuppose certain types of organization of society. Principles relating to women, or to men only, or to wives or to husbands, e.g., the duty of wives to obey their husbands, are universal only in the sense of their relating to a class. And the same is true of principles concerning the duties of citizens, professional groups such as doctors, teachers, etc, although some of these principles are derivative from other principles universal in some fuller sense.

Hare cannot exclude any of these senses of 'universal'; yet, in terms of most of them, we can formulate principles which we should not concede to be moral principles — not even false moral principles. Egoism, principles of manners, rules of taste, certain dictates of fashion — particularly those of a condemnatory character — and even certain precepts in sport, for not all are of a hypothetical character, may be advanced as universal prescriptions without thereby necessarily becoming moral principles.

Relevant here is Hare's discussion of saints. Hare, in terms of his formula, has a lot of trouble with judgements relating to saints, arguing that these judgements are merely conventional. This is not always the case, although it is probably so in the case he cites, that of St. Francis. Hare's analysis would seem to commit him to the view that there is only one way of life that is moral from the standpoint of any one person at any one time But it is clear that we can commend the life and death of Christ or of Socrates, without prescribing the same conduct for ourselves and others. Similarly, we can say of some individuals that they ought to be celibate, ascetic individuals, and of others that they should not be so self-disciplined, and applaud both ways of life, without carrying thereby any suggestion of universality or any self-committal, other than that, if I were *exactly* as he, etc., then that is what I ought to do. But then, many non-moral rational imperatives could have this implication.

Judgements about saints therefore bring out the unsatisfactoriness of the claim that moral principles may be distinguished as prescriptions which are in an obvious sense, and in a sense peculiar to them, universal. At the same time, these same judgements suggest that the sense in which moral principles may be viewed as *self-committing* prescriptions is a sense much more sophisticated than that suggested by Hare as: 'Let me—'. We do not ask to be allowed to do as Christ or Socrates or the ascetic did; nor is the barren woman, born without the essential organs, committing herself—she cannot be—when she makes the Biblical injunction 'Be fruitful and multiply' a moral principle she tries, in her capacity as marriage guidance counsellor, to impress on her consultees. It is true that, by the end of the book, Hare waters down 'self-committal' to merely 'subscribing', but this largely establishes my point.

Thus, unless the senses of both 'universal' and 'self-committing' are explained much more subtly, they do not relate to all that is moral. This means that, as it stands, Hare's theory breaks down in a serious way, in that it does not indicate how moral and non-moral prescriptions are to be distinguished.

6. Again, Hare's account makes moral advice puzzling. How, on purely moral matters, can we instruct the next generation about moral principles, if this analysis be correct? And how is genuine moral advice, as distinct from moral exhortation, possible? Hare can only give very incomplete answers to these questions, in terms of his account of moral principles as prescriptions.

7. Hare's difficulties here expose him to the other traditional criticism relating to the tremendous arrogance of the person who accepts the subjectivist analysis, and who persists in making moral pronouncements. All that Hare's analysis does—by contrast with earlier subjectivist analyses—is to make this arrogance puzzling. It makes it incredible that modest and even humble people should wish to be so arrogant, and to continue to be so, once they have grasped the logic of their moral expressions, for Hare gives not even a hint of a motive, let alone of a motive which would plausibly explain such otherwise uncharacteristic behaviour in modest ethical subjectivists. Earlier subjectivists such as Stevenson had related the use of moral expressions to private attitudes or interests we wish to impose This makes the behaviour of the ethical subjectivist understandable, if unplausible. Hare's analysis, on the other hand, without further additions, simply makes the arrogance of the ethical subjectivist fantastic and non-rational, perhaps even irrational.

Many more illustrations of how Hare's theory fails to overcome standard, traditional objections to subjectivism could be developed here. For example, Hare's difficulties concerning error, immorality and wickedness could be examined at length. and the same general conclusion further reinforced. This is not necessary, for sufficient has already been said to bring out that the traditional objections to ethical subjectivism are serious objections, not easily circumvented; and further that they are such that attempts to circumvent them are likely both to be at best only partially successful, and to lead to equally serious new difficulties, peculiar to the proposed solution. Our examination of Hare's analysis illustrates how this occurs Hare is concerned with the traditional difficulties — in particular with that relating to the rationality of morality. In his efforts to meet this difficulty, he outlines an analysis which raises innumerable, equally serious difficulties of its own; and even then, it is not successful in meeting the traditional objections to ethical subjectivism which his analysis was designed to overcome.

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ON EVIDENCE FOR IDENTITY

By G. C. Nerlich

It is widely realized that there is a danger in the method of considering philosophical questions in terms of invented cases. We must not import into our merely possible world attitudes formed and appropriate only in the actual world. Despite the familiarity of the danger, it often happens that one falls into the trap. I want to underline the danger by considering some cases which are associated with the question whether continuity is or isn't a necessary condition of identity. These cases are canvassed as often as the question is raised and I want to show how inappropriate it is that one should be struck by some features of these cases when the cases are merely possible. For what is striking is striking only as evidence for identity in the world as it is. The confusions are fairly subtle and complex, and since the cases are far more often raised in conversation (where they are less likely to be closely scrutinized) than in writing, error is frequent. I have a secondary aim, which is to defend the thesis that continuity is a necessary condition of identity, at least in so far as the cases I consider might seem to undermine it.

In a sense, I shall argue against a man of straw in that I shall attempt to refute the view that when an object ceases to exist, and later an exactly similar object comes into existence, the similarity has any power whatever as *evidence* for the identity of the objects. I doubt that anyone has ever argued for the view that it does have evidential weight. But, despite that, it seems pretty clear that we are apt to be *struck* by the similarity and it is only striking as evidence for identity. And being struck by it gets in the way of our correctly considering the case as one calling only for arbitrary decision. I am arguing against the "evidence" point of view because I think it *is* a submerged element in our way of looking at the case, and I want to bring it to the surface and refute it as if it were someone's considered position, in order that what I say should be as clear as possible. I want to make out the thesis, in my first part, that an appeal to similarity as evidence would involve two ways of begging the question whether the things are identical, and, in the second part, that if we do allow ourselves to appeal to similarity as evidence, then we have no right to assume that continuity has failed. I shall not consider the logical relations between statements about continuity and identity or between statements about similarity and identity (nor which way we should vote on a case when it calls only for decision).

I.

Let us consider the following persuasive argument against the thesis I am trying to defend. "In very many, perhaps the majority of cases where we have to decide an identity question, we do not check whether a corresponding statement about continuity is true or not but simply assume it. The way in which identity questions are often, in fact, answered is by considering the state of the present object relative to the state of the former object with which it is possibly identical. So that if it is in fact the case that we often settle identity questions without a prior settling of continuity questions, we might, if the world were other than it is, regularly settle identity questions without so much as raising continuity questions by placing our whole reliance, logically, on the evidence we now often accept as adequate. In such a state of affairs, we could not claim that continuity was a necessary condition of identity." In order to defend the thesis from this attack, it is essential to go into the problem of the ways in which we do in fact settle identity questions, how the evidence typically taken into account comes to have its power as evidence for or against identity, and whether continuity is simply assumed.

If we do not consider questions of continuity in settling identity questions, then we can only consider questions of the state of a thing relative to the state of the thing with which it is putatively identical. Our interest in the relation between their states can be of one of two kinds: (*a*) in whether the states are similar in every detail; (*b*) in whether the states are similar in some respects only and in other respects different, though the differences are such that the state of the later thing coincides with what we expect, or might have expected of the earlier one. E.g., if I put a piece of honeycomb in a refrigerator at night and find a piece of honeycomb there in the morning, then its exact similarity with the piece I placed there last evening would count as evidence for its being the same piece and any dissimilarity would count towards its not being the same piece. But if I leave a piece of honeycomb beside the fire for an hour and then return, it is the wax's being in a different state (though in a state coinciding with one I expect) which is evidence for its being the same wax, and if I found a piece of wax similar to the honeycomb I left this would be evidence that it was not the same wax (or that it was the same, but had not been wax or that it had been moved from the fire and been replaced). For the sake of economy and clarity I shall talk of these cases in terms of x's being similar to y for (*a*) above and x's being in a state predictable of y for (*b*), where the names "x" and "y" do not commit one as to identity or difference. It is clear that (*c*) is a special case of x's being in a state expected of y.

The question now arises how it is that these two kinds of evidence merely from the states of things get to be evidence at all. For, as we have already seen, similarity does not exclude difference, nor of course does dissimilarity, even if, in the sense above, it is predicted. Yet it is clear that each, in appropriate circumstances, is taken to be quite powerful evidence for or against identity. There are several features of our world, which I shall try to describe, which account for this. It is important to notice that they are features of *our* world, not of *any* world.

First of all, it is to be noticed that, typically, x's similarity to y or x's being in a state predictable of y works most powerfully for identity where there is prima facie evidence of continuity, or at least no evidence of discontinuity. For the vast majority of kinds of things, we have very good evidence indeed that they endure. We have experience of flames, sounds, etc., passing out of existence, ceasing or stopping, but no experience of physical objects having such ephemeral existence. No such snuffing out of existence has ever been observed and we very rationally "take for granted" that it never does happen. Indeed, that matter is conserved continuously is one of our best founded and most basic contingent general truths.

In the cases mentioned above, the wax is taken from the refrigerator or found beside a fire *where is was left*. It is here that the expected state of a thing has its greatest evidential power. Ordinarily, if there is clear evidence of discontinuity, finding a similar thing does not give rise to a question of identity. Also, if one comes across something y whose state does not coincide with a possible state of x, then no question as to the identity of x and y arises either. If I observe a thing just before boarding a plane at London Airport and see a similar thing when I leave the plane at Paris, I do not normally wonder even for a second if it can be the same thing, unless the similarities are particularly *striking* (and what constitutes a striking similarity will be discussed subsequently). And if the question does arise, i.e., if I do wonder about identity, then

I wonder about how the thing could have got there. But though this is worth noticing, it says nothing to the purpose about evidence in the sort of case under discussion and is far too weak to defend the thesis on its own.

The second way in which x's being in a state predictable of a previously encountered y has evidential weight is due simply to the improbability of our finding two things exactly similar to each other (or another thing exactly similar to what we expect a given thing y to be like). This does not always obtain, however, for often there will be a high probability of multitudes of things resembling each other indistinguishably from our point of view at least, e.g., grains of sand, coins, cubes of sugar, etc. Here we should distinguish two things, (*a*) whether it is improbable that two grains of sand or two pennies should *exactly* resemble each other, and (*b*) whether it is improbable that we should find two pennies or two grains of sand so similar that we should not, in practice, be able to distinguish them. There is some confusion over (*a*) due to the chronic troublemaker "exactly". but I do not see how anyone can estimate the probability of there being or not being two grains of sand exactly similiar in all respects (once we decide what we mean by this), for there are so many grains.[1] In any case (*a*) is less important here than (*b*), since we are considering ways in which we do *in fact* settle identity questions and it is clear that it is not in the least improbable that we should not be able to discover dissimilarities between some similar things. And where this is so, as it is with coins, we must and do enquire carefully into the question of, e.g., where a coin was lost, and where found. It is, of course, much less probable that two indistinguishable coins should be in roughly the same place at roughly the same time than that there should just be two indistinguishable coins somewhere in the universe. The state of a thing alone, therefore, has evidential power so far as probabilities are concerned according to (*a*) its complexity and the consequently large number of ways we may make comparisons between it and other things; and according to (*b*) the scarcity of objects likely to have these properties; and it is clear that these two factors will often be connected. This suggests that there are, in fact, many cases in which it is most convenient to combine the evidence of similarity with partial (though good) evidence of continuity.

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[1] Let us assume that two objects x and y fulfil this condition: For any possible standard, S, of comparative measurement of any property or set of properties, x and y are not distinguished by S measurement

At this point let us describe one of the typical cases from which an appeal against the necessity of continuity is made. Suppose I own a cube of marble which has a distinctive and intricate pattern in its grain. As I watch it, suddenly it vanishes from sight, nor can I detect it by any other means whatever. Surely, we may say, it has ceased to exist. Later, a cube of marble exactly similar in every way is suddenly seen just where the cube vanished. Surely, we may say, this is the same cube again. And if we do say this (and it is very tempting to say it) we should be saying that continuity is not necessary for identity.

There are striking analogies (despite equally striking disanalogies) between features of this invented case and ordinary cases in which we are prepared to assert identity on the evidence of similarity. These analogies deserve attention. First, though discontinuity is evident here, it is of an extraordinary kind, i.e., there is a temporal gap. For the putatively identical thing is encountered just where we would have expected to encounter the object had it continued to exist. That is to say, it is, in another way, *very* like the situation in normal cases where similarity has its greatest evidential power, viz., those cases where there is *prima facie* good evidence for continuity before similarity comes into play, so to speak. It is not at all like those cases of *spatial* discontinuity where one sees a bottle at London Airport and an exactly similar bottle in Paris Airport at each end of a fast plane trip. It becomes very easy, when we deal with cases of this temporal sort, to treat the evidence of similarity just as we would treat it in an ordinary case where continuity is preserved. This point will be seen as more substantial relative to the second sort of error discussed in part II of this article.

But more important than this is the *imaginary* nature of the case introduced. It is not therefore at all a straightforward matter to suppose that correct attitudes to evidence in ordinary cases will be appropriate in such extraordinary cases. And if we examine that aspect of the evidential power of x's being in a state predictable of y which comes from probabilities in the way discussed above, we shall find that to carry our present attitude over to this imaginary case is to beg the question at issue. For suppose one flatly denies that x and y can be identical. Then one may readily grant that their non-identity is wonderfully, strikingly improbable. It is, indeed, simply incredible that there should be two cubes so intricately alike, so close together in space and time. But this should not tempt one to give the improbability *any weight whatever* in deciding to say whether the cubes are the same or not. "For", one may say, "to appeal to the improbability of their difference is just to appeal against allowing the case at all. On my view of the relation between continuity and identity, the improbable occurrence (viz., *two* exactly similar cubes) was just what you were asking that I should imagine. To lay stress on the improbability of there being two at this stage is either to reject the case, which I shall do if you wish, or to beg the question against me by assuming that the cubes *can* be identical, i.e., by assuming that continuity is not necessary for identity."

This does not wholly explain the status of similarity nor the status of x's being in a state predictable of y as evidence for or against identity. In order to clarify this question, it is necessary to give some account of the relevance of identity to cause and change.

In our investigations into the nature of things, whether at crude or sophisticated levels, it is required that we should have ways of telling whether at one time we have the same object as we had at an earlier time; ways which do not depend on relative states of things. For if it is required to establish what changes a process P brings about in an object x, or whether Fx's can be changed to Gx's by process P (i.e., whether process P is the cause of Fx's becoming Gx's), then we shall require methods of proving that the x I now have is not merely the thing with which I began but also that it is *the thing which underwent P* (i.e., the x which was *continuous* while P lasted), and it is clear that x's state relative to y cannot be used here, since, until we have discovered the effects of P, we shall not be able to predict the future state of x (and its remaining in a similar state is only a special case of a predictable state). Often we can establish the identity and continuity by continuing to observe the x as it undergoes P, but perhaps equally often this will not be possible. However, there are, as has already been mentioned, good grounds for the generalization that physical objects are enduring, that matter does not snuff out of existence as candle flames do, and as well as this we have well founded laws of motion, etc. Both these types of generalization are used in tracing the paths of things and both in turn are extrapolations from the results gained by watching continuous stretches of the histories of things. It is clear from this that we find out about causes, the effects of processes and the natures of stuffs by learning to establish continuities and identities by means quite apart from the states of the things involved. We can predict the future state of a thing from a present state of it combined with the conditions of its environment, only because we can observe stretches of continuity or because we can observe the beginnings and ends of stretches which, we can establish, do continue between and beyond the observed stretches, even if we cannot observe the inferred stretches themselves. In any case we must first observe stretches of continuity before we know the kind of circumstances in which we will expect a thing to remain constant in its properties (and so allow similarity as evidence for identity) and those in which we will expect a thing to change, as well as the kind of change to expect, in its properties (and so allow y's being in a state predictable of x as evidence for identity).

It should now be clear that the persuasive argument presented at the beginning of part I somewhat misrepresents the facts. For while it is true that we do regularly settle identity questions by considering the state of x relative to the state of y, it by no means follows that we are thereby neglecting questions of continuity. It is only by consideration of continuities that observations about the state of x relative to the state of y have any status whatever as evidence for deciding identity questions.

Further, this discussion will now enable us to see how it is that certain features of the state of x qualify as *striking* evidence for its identity with y. We often distinguish otherwise very similar objects by marking them, or by their being marked, by some unique process. A man may put a distinctive scratch, e.g., his initials, on the shaft of a golf club, or it may be that his car's mudguard is welded in a certain place after an accident, or he may write his name in a book. Let us call such distinctive marks traces, since it is a contingent truth of great generality and evidential power that such precisely specifiable marks appear on objects *only* as a result of their being subjected, at some time, to a quite definite process. That is to say, it is a contingent universal truth that objects do not come into existence already bearing such marks, for it is a (causally) necessary condition of having such a mark that a thing should include in its life history having been subjected to process P. Traces, therefore, will be singularly powerful pieces of evidence for identity both because it is so improbable that two objects should ever be marked in just this distinctive way, and because we are sure of this contingent general truth. An object which bears a trace must, therefore, have existed before our present observing of it.

Let us now consider another case, which includes the use of a trace as evidence for identity. What we are here being asked to imagine is that an ink-bottle with a man's initials written on the label ceases to exist and that later an ink-bottle in no discernible way different comes into existence with exactly similar lettering on its label. This is a striking and powerful piece of evidence that it is the same ink-bottle come back, or that, at the very least, this ink-bottle has existed at *some* time before its present materialization, i.e., that it is identical with *some* ink-bottle on which *some* man formerly wrote his initials. But the force of this is due to the contingent general truth about traces. However, one cannot invoke this general truth in order to give emphasis to the evidence of the initials on the label in the present case, without begging the identity question at issue in a way very like the way in which stressing the improbability begged it. For one might say that, in being asked to imagine this case, one is being asked thereby to imagine a counter-example to the contingent general truth. For only if the ink-bottles are identical can it be claimed that the general truth is not falsified. Unless one already agrees that they are identical, then one is being required to imagine that something *does* come into existence with a trace on it, but which is *not* identical with anything which includes in its history being subjected to the process which produces that trace.[3]

So in these invented cases we cannot give *any special status whatever* to any aspect of the state of a thing as evidence for its identity with some earlier thing, without begging the question. To be struck by these cases, as one almost invariably is, already prejudices our decision on them.

The argument introduced at the beginning of the article is therefore specious and incorrect, though persuasive. For, as has, I hope, been sufficiently established, it fails on the one hand to do justice to the facts in overlooking how questions of the state of x relative to the state of y do not supersede, and are not (in terms of this sphere of enquiry) independent of, questions about whether x and y are continuous. And it also presupposes that an attitude to evidence which is appropriate in a world where things continuously endure will be equally appropriate in a world where they do not. This assumption leads the arguer to beg the question in the two analogous ways described.

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[3] Memory is a kind of trace (J. J. C. Smart: "Temporal asymmetry of the world", *Analysis*, March, 1954, p 80). What I have to say against traces as evidence for identity in these imagined cases also applies against attaching weight to memory in the analogous personal identity cases The frequent charge of circularity urged against using memory as establishing identity, though it is correct, ignores the overwhelming power of one kind of knowledge of the past as evidence for identity in the ordinary case But this power depends upon its being universally true that only by being identical with a witness can one come to have that kind of knowledge of the past. And this is a universal truth of *our* world, not of all possible worlds. I do not wish to claim that, in the case of persons, identity need be body-dependent, so perhaps *spatio*-temporal continuity is here dispensable. But some sort of continuity is essential; a person cannot wholly cease to be, and later live again.

II.

This not merely disposes of the argument, but it also enables one to see more clearly through another cloud of confusions, which obscures the issue. In what follows, I shall try to show that though we can regard the cases usually introduced into this problem as decision cases, we cannot in any straightforward way regard them as decision cases related to the question whether continuity is a necessary condition of identity. My aim is to show that the more we become impressed with the feature of similarity in the case, the more we are tempted surreptitiously to suppose that x is continuous with y. It is not my intention to defend a sceptical position as to the relation between observation reports and negative existential statements, or to suggest that it would be *unreasonable* to say in the cases described that x had ceased to exist and y had later begun to exist, but to argue that, if we do say this some attitudes to the case thereby cease to be appropriate.

First, what is a decision case? A decision case is one in which all the facts are in our possession, but what remains unclear is what we should decide to say about it. Should we say, in Wittgenstein's well-known case,[8] that the dog does, or does not, go round the cow? In decision cases it is clear that little hangs on what we say so long as we know (as we do know) all the facts. There is a clash between criteria for asserting F or not-F (or G), but there is no question of our being *ignorant*. But is the ink-bottle case one in which we know all the facts? It is not at all clear that it is.

Philosophers, when they discuss this case, take it for granted that there is no difficulty, as the case is described, about the existential statements which must be true before we have a case of discontinuity. In doing this they presumably have in mind the fact that when, ordinarily, we search a room thoroughly and find no ink-bottle, we are entitled to say there is no ink-bottle in the room. Similarly, when we search the world and find no winged horse, we are entitled to say that Pegasus does not exist. And in assuming that it is a perfectly straightforward matter that the ink-bottle ceases to exist, they are paying attention to one part of the evidence, while neglecting to consider it as a whole. This must now be done.

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[8] *Vide* J. Wisdom, "Metaphysics and verification", in *Philosophy and Psycho-analysis*, pp. 95-6.

It is important to notice that cases in which the confusion I now wish to clarify occurs are described in terms of a predicament in which we might find ourselves puzzled owing to our human limitations. We are told only what is observed by a person in this situation. We are not given a set of statements about existence and similarity such that we can test the logical relation between them and a statement of identity or difference. We are only given the evidence on which statements about identity and existence might be made.

If we are to examine the case evidentially we must first consider the question whether we shall be justified, given the observations described, in asserting that the ink-bottle ceased to exist. Statements of non-existence are, some might claim, impossible to verify.[4] This view, though not entirely without substance, is mistaken in part and we must notice some exceptions to it. The reasons for holding this view are that, for positive existential statements, e.g., "Pegasus exists", we can specify an observation-result, or set of them, such that if we obtained the result or set of results at any one time and place we should have verified the existential statement. That is to say, we can specify a result such that, if we achieved it at the first observation, we could cease to search. This is not true of negative existential statements. This does not mean that all positive existential statements are, in fact, always easier to verify than negative ones. For, obviously, the same search will be required to verify "Pegasus exists" as to verify "Pegasus does not exist" and, assuming there is no winged horse, any set of observations will be inconclusive. The difference is only that we can *specify* a successful observation for the one, but not for the other, for there is no finite number of places (and so no specifiable *set* of places) such that an observation-result in all would verify the negative statement, and there is no observation-result such that if we obtained it first we could cease our search.

But there are three kinds of case of which this is not true, exemplified by the statements "Oxford does not exist", "Napoleon does not exist" and "No one is in the room". In the first case we can specify a place in England where we can observe and if we find no buildings in that place we shall have verified the statement. In the second we can find bodily remains of what was clearly the man and establish, by the state of the remains, that he has been destroyed, i.e., has died. In similar cases, we can specify an observation result (finding a body, the rubble of a bombed building, the ashes of a box) such that if we achieved it at any one time we should have verified the negative statement. The third case, like the first, sets a convenient boundary to the area of search and finding nothing answering the given description within that area verifies the negative existential statement. But, these exceptions apart, it remains tenable that negative existential statements are unverifiable. It is obvious that, for the present purpose, these three cases are of no importance, for in the cases we are considering, where a thing is not observed where it formerly was, the negative existential statement in question does not fall within either of these three classes of verifiable statements. Except perhaps within the third class, but here the situation is complicated by the fact that the object *did* exist, and in this room. While, in fact, there are commonsensically limited requirements for what is to constitute establishing the non-existence of a Pegasus, it is not at all clear how to limit the requirement for establishing as acceptable (if not *proved*) the particular negative existential statement under discussion. For things just do not pass clean out of existence. The ink-bottle might always continue to exist, but have moved *to another place,* but too quickly for us to notice, etc.

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[4] It is doubtful if anyone would make so strong a claim as this. But see Russell on Wittgenstein, *Mind*, 1951, p. 297.

It will be objected that this ignores the point that the evidence given in the case may be very powerful, as good as we could ever have, for saying that the thing does not exist at the specified time, and that the power of this evidence is not lightly to be neglected. This is a valid objection only so long as we do not consider the evidence presented by the case as a whole. For what complicates the question and calls in doubt the negative result of the observations made in the "time-gap" is the later appearance of an ink-bottle exactly resembling the one which disappeared. Things no more come clean into existence than they pass clean out. The case now becomes so peculiar that it is very hard to see what ought to be said about continuity and existence as well as about identity.

One reason for this cannot be too much emphasized and repeated. It is a very well established truth that objects endure, and so powerful is our belief in this that the hypothesis of some kind of disappearance and reappearance in such cases is hardly to be neglected as being more fantastic than the hypothesis of ceasing to exist and materialization. This does not mean that we *cannot* be asked to imagine that a thing has ceased to exist and a thing has come into existence. But, as was shown in the first part of the paper, this puts us in a position where some attitudes to evidence are inappropriate. The attraction of putting the case in terms of our human predicament is that, given our world and our present attitude to evidence, the evidence here is so very powerful for the identity of x and y.

This suggests another related reason why it is hard to see, in such a case, what we ought to say about continuity as well as about identity. In our world the very same evidence which establishes identity also establishes continuity. So that, where similarity is powerful evidence for identity, there is also powerful evidence for continuity.

It might be objected at this point that this argument is inconsistent with my earlier argument, that establishing identity by means of continuity is prior to similarity's being evidence for identity at all. However, the inconsistency is only apparent. The first argument supposed we did not know about causal processes, i.e., know either the circumstances in which the properties of various kinds of things remained constant, or those in which they changed, or how they changed when they did change. It urged that the only way of coming to learn this was by observing stretches of continuity. It is a necessary condition for the state of x relative to the state of y becoming evidence for or against their identity, that we should have gained this knowledge ultimately in this way. The second argument presupposes our having such knowledge and sees similarity as evidence for continuity (wherever it is evidence for identity) against a background of such learned facts. So the arguments are not inconsistent, as might have been supposed.

Another objection might arise here. If we settle identity and continuity questions in the light of the very same evidence, then it may be the case that the evidence establishes identity and there is an *inference* from this to continuity. This would mean that the identity question was prior to the continuity question and not that the former was decided in the light of the answer to the latter. This is mistaken, for there is but one kind of inference which could be involved here, viz., a *deductive* inference. So that if identity, according to this objection, is a logically sufficient condition of continuity, then continuity must be a necessary condition of identity. This grants the point and so is no objection. No other suggestion as to the kind of inference will do, for it could not be a simple induction, since it is just false that every time we have established identity, we have (independently) *observed* continuity. Nor would it be at all plausible to argue that continuity was a cause of identity. This would be utterly absurd. So it remains true that the state of x relative to the state of y is evidence for continuity in all those cases in which it is evidence for identity.

In this case, therefore, there is a clash of evidence as to what we should say about continuity and identity. The evidence of the negative results of the observations during the "time-gap" suggests both that the ink-bottle ceased to exist and that the ink-bottle which is later observed is a different one from the first. The evidence of similarity between the thing observed before the gap and the thing observed after it suggests that it is the same ink-bottle and that it merely disappeared or had been in very rapid motion, etc., during the gap. It is by no means straightforward to decide whether the object had ceased to exist, since the evidence taken as a whole is not univocal for non-existence. In so very bizarre a case, it is quite true to say that we should be very exercised indeed about the question of identity, but it is also true that we should be generally baffled as to how to go beyond the given data in any way whatever. We may, if we wish, say that such cases call for conventional decision. But no decision is called for on *this* question: Is continuity a necessary condition of identity? For this question, the cases must be described in terms of statements about continuity of existence, not in terms of evidence for the statements. And when the cases are so described, no legislation is called for, since the structure of language already contains the answer.

It should be noticed that one need not be embroiled in a phenomenalist-sceptical controversy in order to urge the foregoing arguments. It is not being suggested that there is necessarily and chronically a logical gap between statements of what is observed (i.e., statements about sense data, for example) and negative statements of existence. If it were, the argument would apply *mutatis mutandis* to the statements of what is observed supporting the positive existential statement. For the very fact that the "thing" disappears for a time might be thought a good reason for saying it was never there at all, and that the whole thing was an hallucination. And it is, of course, true that the cases need not be so very different from the ones we have dealt with for this attitude to be as appropriate as any. But I do not wish to urge such a view of the question. What is important is to see that, besides this, it is as rational as any other hypothesis to suppose that the thing was somewhere else when we failed to observe it.

The confusion examined in Part II may be summed up as follows. In cases such as this, there is a tendency to set up a tension between the *evidence* of similarity as against the *statement* of discontinuity. But the statement of discontinuity is not a piece of evidence on the same level, logically, as the observed similarity. For the statement is to be made *in the light of* the evidence, which includes the similarity as a part. It is a thorough confusion to create a clash between a statement and part of the evidence taken into account in making it. As was argued in the earlier part of the article, once we have reached the stage of making the statement of discontinuity, the observed similarity has ceased to have the evidential weight which it is so tempting to give to it.

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CONDITIONING AND FREEDOM

By K. J. Scott

Motivational research, hidden persuasion, conditioning by subliminal advertising, are on our minds the way 1984 was a few years ago. But there is something untidy about these new menaces. The villains are not sinister enough. The Madison Avenue boys in grey flannel suits that every country has are good novel material, good film fodder, but not for 1984ish parts. They seem so innocuous — unless we decide that their naiveté, their social irresponsibility, is itself sinister. Devils are predictable, but sorcerers' apprentices are not. We cannot guess what might happen to us if we became the puppets of subliminal advertisers. In one way it would be worse than 1984. We would have things done to us unbeknown to us. Conditioning is an affront to human dignity. It robs us of our freedom to live our own lives in our own ways. Conditioning and freedom are antithetical. At least, we tend to think they are. Thus Aldous Huxley, in a television interview that was reported in *The Listener* a few months ago, said that freedom is diminished by propaganda as used by Hitler and by American advertisers, and he went on to assess the danger to freedom that is implicit in subliminal advertising. What I want to do is to examine this way of thinking about conditioning. What I am going to suggest is that when we see the issue of conditioning in terms of freedom we are using the term 'freedom' in a new way. Now if that were all it would be hardly worthy of notice, seeing we are constantly using words in new ways. But it is not all. This new usage has got accepted without being recognised as an innovation. And unless we see how the new usage differs from the old we shall be in danger not only of thinking confusedly about conditioning but of muddying all our thinking about freedom.

Compare some of the other new ways the word 'freedom' has been used in the past century or two. There have been plenty of them, and that is not surprising, for with excessive government the characteristic danger of modern times, and freedom the dominant social ideal, it is only to be expected that all sorts of social reformers should claim that the reforms they advocate are new ways of realising freedom. We have this when Rousseau says we can be forced to be free, when Green says that freedom is not just the absence of restraint but is a positive opportunity to do things that are worth doing, when Franklin D. Roosevelt gives the name 'freedom from want' to the provision of an economic minimum. Now in all these cases there is no suggestion that the word 'freedom' is being used in its primary sense. In each case a new secondary meaning is being quite frankly added. Rousseau is being consciously paradoxical, Green is being consciously analogical, Roosevelt is being consciously rhetorical. These innovations in usage provide illuminating ways of seeing old problems in a new light. Their very effectiveness depends on the contrast between the primary meaning and the new secondary meaning. But when we say that conditioning impairs freedom we are ostensibly using the word in its primary sense, and that is where the danger of confusion comes in.

For we are not using the word in its primary sense. In its primary sense, freedom is the absence of restraint. When we talk about freedom we take our desires as data, and ask whether any restraint prevents us from giving effect to them. But when we talk about conditioning we no longer take our desires as data. We ask where our desires came from. And if we talk about conditioning in terms of freedom we are confusing two separate problems: the philosophical problem of free-will and determinism; and the social problem of freedom and restraint. The philosophical problem is about the internal causes of our behaviour; are we determined to react to our present environment in a particular way? This problem about the internal causes of our behaviour is not concerned with any restraints there may be on our freedom. The social problem is about some of the external causes of our behaviour; are we restrained from acting in a particular way? And this problem about restraints is not concerned with whether we possess free-will. It is easy to see how conditioning comes to be considered in a confused way. Conditioning is like restraint in being something external, and so it comes to be thought of as posing the same sort of problem as restraint. But conditioning operates by altering our internal make-up, and thus becomes an internal cause of the way we react to our environment. The problem it raises is the philosophical problem of free-will, not the social problem of freedom — though, of course, it raises other social problems. There is another way of distinguishing the subject-matters of these two problems that makes the point even clearer. The philosophical problem is concerned with the anterior causes of our behaviour, and the social problem with some of the current causes of our behaviour. Conditioning is an anterior cause. Conditioning and restraint are unrelated.

I said that freedom is the absence of restraint. Perhaps a word of defence is needed. For this kind of definition is sometimes criticised as a negative, nineteenth-century definition. There is a school of thought that says that freedom is opportunity, means, ability, power. This is to out-Green Green, and take Green's secondary meaning as being the primary meaning. The answer to this kind of view is to be found by examining common habits of speech. When the word 'freedom' is being used to mean the absence of restraint, it falls on the ear as the right word to use. When it is used to mean opportunity, means, ability, power, there is always an air of paradox. We know that some sort of point is being scored when are told that the poor man is not free to dine at the Ritz, that the ordinary citizen is not free to be a press magnate, that there is no freedom for a woman to become prime minister. The idiomatic way of voicing these social criticisms would be to lament not the absence of freedom but the absence of opportunity, means, ability, power. Departures from idiom do not alter the primary meaning of the word 'freedom'; they only illustrate one or another of the word's secondary meanings.

But to say that freedom is the absence of restraint is to offer a label rather than a definition. For one thing, 'restraint' could be taken to be a purely environmental term, and freedom is not something purely environmental, but a relation between a person and his environment. We should say, more fully, that freedom is the absence of restraint on doing what we desire to do. Restraint operates through deterrents and inducements. We are deterred by threats from doing what we would otherwise do, or induced by threats to do what we would not otherwise do. Sometimes there is only the deterrent, sometimes only the inducement, and sometimes we are both deterred from doing one thing and induced to do another. 'Restraint' is not as good a term as 'constraint', for 'restraint' suggests only deterrents, and 'constraint' is a broader term suggesting both deterrents and inducements. So from now on I mean to write of constraint.

Not every threat succeeds in constraining us. We may choose to ignore a threat. We may think the risk is worth running, perhaps because we think the unpleasant consequences threatened are relatively unimportant, perhaps because we think they are not very likely to happen. We are not deterred by the prospect of a penny fine on an overdue library book, we are not deterred by a law that we know has become a dead letter. If we ignore a threat, then we are *ipso facto* free despite the attempt to constrain us — though we may have to endure the consequences. The people who are especially resistant to threats have the widest freedom, though they may have to suffer for it: heroes, criminals, saints. On the other hand, people who yield to a threat because of morbid timidity probably should not be regarded as being constrained. They should be regarded as being free, though as choosing to exercise their freedom in an odd way. For the effect of a threat to constitute constraint, we must yield to the threat, and we must be acting sensibly in yielding to it. We are constrained only when we are reasonably deterred by a threat from doing what we would otherwise do, or reasonably induced by a threat to do what we would not otherwise do. Constraint operates by creating a new desire, the desire to avoid the unpleasant consequences that are threatened, and thus weakening the relative strength of our spontaneous desire to do what we would otherwise do. The social problem of freedom and constraint is concerned with the creation of the new desire and the weakening of the spontaneous desire. So it was an over-simplification to say that the social problem of freedom and constraint takes our desires as data. What I should have said was that the social problem of freedom and constraint takes as a datum the spontaneous desire to do what we would do in the absence of constraint. The social problem is concerned only with whether we are constrained from giving effect to this spontaneous desire, and is not concerned with whether it had its origin in conditioning. Conditioning has nothing to do with freedom.

I was saying that constraint works through threats. Only threats can constrain us. The prospect of reward cannot constrain us. We can see this if we compare two ways of persuading our son to mow a lawn. If we say that if he does not mow the lawn he will not get his half-crown pocket-money, and he submits to the threat, he mows the lawn under constraint. If we say that if he mows the lawn he will get an extra half-crown in addition to his pocket-money, and he accepts the offer, he mows the lawn freely. One way is approximately as effective as the other in getting the lawn mown, but we would say that in the first situation he is constrained and in the second he is free. Suppose we offer him five pounds instead of a half-a-crown. He would be certain to accept the offer. We could be more sure of controlling his conduct by offering him five pounds than by threatening to withhold half-a-crown. That is, in this case we could be more sure of controlling his conduct by an approach that does not impair his freedom than by one that does. Control works through many mechanisms and of these constraint is only one. I suspect that people who regard conditioning as impairing freedom base this view on an unspoken major premise that whenever we respond to control we are by definition under constraint. But this major premise is false. There are many forms of control that do not involve constraint. Conditioning is one of them.

Another distinction between constrained conduct and conditioned conduct hinges on the nature of the choice we make. Usually when we are constrained we yield to the threat reluctantly, and we feel frustrated. We sometimes feel the same way in other situations where we have to make an embarrassing choice. But we are not likely to feel this way merely because we are giving effect to a spontaneous desire that had its origin in conditioning. Conditioned conduct not only is not constrained conduct but does not feel like constrained conduct.

Scholarly writers on the nature of freedom have given little attention to the question whether conditioning impairs freedom. They talk about post-hypnotic suggestion as an instance of determination, and do not enquire whether it is also an instance of constraint. But Professor Ayer did deal with indoctrination in an article he wrote for *Horizon* in 1944. He began his discussion by stating a *prima-facie* view. He said that in a completely planned society whose members were trained from birth for their respective functions, and were so thoroughly conditioned that they never conceived any desires but those that were appropriate to their station, the subjects would seem to themselves to be free since they were granted the ability to satisfy their desires; but we, surveying the whole system from the outside, would judge, without hesitation, that they are not really free. Then he criticised this *prima-facie* view. He said that if young people can be conditioned by their environment, then young people in England too must be moulded by conditioning, though for the planned action of a ruling caste there is substituted the largely unplanned action of parents, nurses, schoolmasters, writers, politicians, lovers, and friends. This is apparently the *reductio ad absurdum* of the *prima-facie* view that indoctrination impairs freedom. If conditioning is effective, and impairs freedom, then we are none of us free, for, as Professor Ayer says, if we had enough information about our antecedents, we could always find a reason for our acting as we do, and this chain of reasons must eventually carry us to facts that are in no way attributable to ourselves. Professor Ayer finished rather inconclusively. On the one hand he endorses his criticism of the *prima-facie* view, saying that, if we believe that antecedent determination impairs freedom, it is illogical to apply this belief to planned conditioning and not apply it to the whole of human conduct. On the other hand, he says that any faithful account of the current conception of freedom is bound to take account of the *prima-facie* view that skilful indoctrination impairs freedom. I feel that if Professor Ayer had been analysing the nature of freedom instead of analysing the current conception of freedom he would have finished with a firm statement that it is illogical to combine the views that conditioning impairs freedom and that human conduct is ever free.

I conclude that subliminal advertising cannot impair freedom. This one particular criticism of subliminal advertising is based on confused thinking. It may be that there are other serious moral objections to it. I think there are, but they lie outside the scope of my subject.

Finally I want to consider how we can have come to believe that conditioning impairs freedom. I think the answer is to be found in our attitudes to totalitarian regimes over the past generation. We have criticised these regimes for withholding freedom from their citizens. At first all that we meant was that the opponents of the regimes had to conform or suffer the consequences. Once we had got into the habit of thinking of these regimes as withholding freedom, we were faced with the problem of reconciling this with the fact that millions of loyal supporters of these regimes were glad to have an opportunity to obey the rules under which they lived. The only way to interpret this phenomenon as constraint was to say that indoctrination is a denial of freedom. And now we have come to believe that what is true of indoctrination is equally true of other kinds of conditioning, and we say that subliminal advertising is a denial of freedom. My thesis is that this view is based on a confusion of two issues that philosophers have succeeded in keeping separate for centuries: the philosophical question of free-will and determinism, and the social question of freedom and constraint. It would be ironical if our generation, in which so many philosophers see the task of philosophy as being a task of clarification, should be the generation that confuses these issues. Conditioning is determination, but it is not constraint.

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PHENOMENALISM: A SURVEY AND REASSESSMENT

By J. E. McKinney

I. *General Considerations*

It is a sobering reflection that, had the phenomenalist been able to make good his claim to have discovered the essential nature of the thing of our everyday world, and by corollary, of course, the nature of the world itself, he would thereby have instituted a complete revolution in our conception of knowledge: in our conception, that is, of our relationship as rational beings to one another and to our external environment. And the significance of this lies in the fact that what has been called "the modern crisis of feeling and of thought" consists in the failure of the traditional sense of that relationship. It is not so long since thought and action, despite the inherent diversities of individual outlooks, cohered within a commonly accepted conception of both rationality and reality. Today that ancient assurance has gone. We are all thrown back on our own resources. Of necessity — a painful and bewildering necessity to many — we are all moral and intellectual opportunists, feeling our way from one situation to another, hoping for the emergence of some new assurance, some new and all-comprehending way of looking at things.

It was just such a revolutionary new way of looking at things that was implicit in the phenomenalist's claim. For what that claim amounted to was that the world, which throughout the history of thought had been accepted as the self-subsistent *cause of* the sort of experience any normal individual had, was in fact a *construction from* that individual's own experience. If the full implications of this are faced, there is nothing left of our traditional ways of thought — startling as this may at first sound. This is so because, on the phenomenalist view, we do not face a self-subsistent world whose structure we are gradually getting to know, but a world which is a structure of experiences. If the phenomenalist were right, it would be this experiential structure that we were getting to understand. We would not be getting to *know* the *world,* but getting to know how knowledge itself has arisen out of experience.

But the phenomenalist failed to make good his position. After some preliminary and somewhat perverse thrusts, in which he set out to shock the traditionalists by asserting that "things didn't exist" or that they were "mere fictions", it emerged that the data of any individual's experience was insufficient to account for the world-picture which that individual actually possessed.

As Ayer has expressed it, "If the phenomenalist is right, the existence of a physical object . . . must be a sufficient condition for the occurrence . . . of certain sense-data . . . and the occurrence of the sense-data must be a sufficient condition for the existence of the physical object"; and, he suggests, neither of these conditions is in fact fulfilled.[1]

But this criticism raises a peculiar difficulty. As we have seen, if we accept the full implications of the phenomenalist standpoint — and that is the only legitimate approach to the problem he has raised — what has traditionally been accepted as a self-subsistent object which is the cause of the individual's experiential data is, on the contrary, a construction from his experiential data. So, if the phenomenalist is right, it would not be possible to trace the steps from the self-subsistent "object" to the individual's sense-data, or from the sense-data to the "object", because, by definition, the "object" is not an entity of that self-subsistent order. In which case, though the failure to trace such steps might not be sufficient proof that the phenomenalist was right, it would not be proof at all that he was wrong. Whatever may be the nature of the connection between "sense-data" and "object", if the phenomenalist is right it will not be of the kind assumed in the above criticism.

But this itself has a peculiar implication. If the above criticism is the principal ground for refuting the phenomenalist's claim, then the phenomenalist's claim has not in fact been refuted. If the phenomenalist is wrong, he is wrong for some reason that has not so far been adduced. This seems to require us to look again at the phenomenalist's claim.

II. *The Object*

A significant feature of the "object" which has been the subject of the phenomenalist's analysis and redefinition — I think we might say, its essential feature from our standpoint as experiencing beings — is that it is common to all of us. Indeed, to function as an object, a something of which we have knowledge, it must have the characteristic of objectivity, it cannot be subjective and personal. The pink rats which a man in the "horrors" sees crawling up the wall are not objects in this sense — they are purely subjective denizens of his own peculiar and private world. The story is told of a man in such a state who said "Never mind, I've got a mongoose to catch the rats". When it was pointed out to him that they were imaginary rats, he said, "Well, so is the mongoose". There is an essential distinction between the subjective and objective worlds.

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[1] A. J. Ayer: *The Problem of Knowledge*, pp. 124-5.

As objective or common, the thing of our everyday experience has this further significant character, that it functions as a common *form* toward which, as individual experients, we orient ourselves.

A thing, say a tree, acts as a form in terms of which the individual organizes his experience. This is so whether we regard the tree as an eternal pattern stored up in heaven, as a function of the structure of the human mind, or as a self-subsistent entity "out there" ; whatever view we take, the tree is a common form which acts as the organizing principle for the experience of all individuals.

The question thus is not "Are there forms governing the individual's experience?" but "What is the nature and the origin of the forms that govern the individual's experience?".

It would seem, then, that the real flaw in the phenomenalist account of knowledge arises, not from the impossibility of relating "physical things" to sensory data, but from the impossibility of deriving the *common* form "thing" — common in the sense that it is valid for all experients — from the data (whether sensory or otherwise) of the individual's *private* experience.

Let us stand back from this difficulty and take an over-all view of the situation we are involved in.

The phenomenalist view of knowledge is a development of the empiricist tradition which regards knowledge as a function of experience. To this general empiricist thesis there does not seem to be any alternative, unless we are prepared to fall back on some kind of extra-experiential factor or principle to help us out.

Empiricism says: All knowledge is a function of experience. But experience is an abstract term. In practice it necessarily means individual experience. And it is in the nature of the case that individual experience is peculiar and private to the individual.

Phenomenalism says: Knowledge is a function of or a construction from the data of individual experience. Phenomenalism makes explicit what was implicit in empiricism. Indeed, looked at in this way, phenomenalism is not a modern theory of knowledge which the phenomenalist has arrived at on his own intellectual initiative. It is simply the logical implication of the empirical standpoint, of which the development of thought has made him aware.

The implication of all this is rather drastic. We cannot adhere to the empirical standpoint and reject phenomenalism, because the latter is simply the logical working out of the former. For this reason, it would be better to drop the term "phenomenalism", with the confused complex of associations that have gathered around it, and speak simply of empiricism.

But the implication that immediately concerns us is that the phenomenalist view of knowledge, as currently formulated, lands us in a contradiction: the only and necessary source of knowledge is the data of individual experience, but the experience of any individual we examine is insufficient to account for the knowledge that individual is found to possess.

There is a still further difficulty which may be glanced at here. I refer to the so-called "Egocentric Predicament". It has peculiar implications for the empirical theory of knowledge, which are easily overlooked.

For the curious feature of the Egocentric Predicament is that, if it existed, we would not be able to say that it did. Conversely, the fact that we can and do say that it exists, the fact that we are able to communicate our ideas concerning it to one another, is *prima facie* proof that it does not exist. The predicament is usually formulated in this way: The knowledge I possess must have been derived by me, by way of construction or inference, from my own private data. Even the knowledge I receive by way of verbal or other instruction must come to me in the form of words or other symbols which, ultimately, present themselves to me as data of my own consciousness. Or, simply, I can experience only my data, you yours, and never the twain shall meet. Stated in this form, the Egocentric Predicament seems to commit us to a fundamental solipsism from which there can be no escape.

This looks like a deadlock. But it can't be. If the above account of knowledge were correct knowledge could never have been arrived at. But knowledge *has* been arrived at. We *have* knowledge. Furthermore, knowledge means common knowledge: what is not common is not, in any strict sense, knowledge at all, as the case of the pink rats made clear. Theoretically, we are all prisoners of the Egocentric Predicament; actually, the predicament has been overcome in the body of common forms we call knowledge.

So the question facing us is: How, despite the so-called Egocentric Predicament, has common knowledge been derived from the data of private individual experience? Or, alternatively: How have private experiential data been raised to the level of common communicable knowledge? We know that they have been so raised. The question is, how?

Phenomenalism, in the form in which it has been accepted by its adherents and rejected by its critics, has been unable to tell us this. But on consideration it is seen that the Phenomenalist makes *two* claims regarding the thing of our everyday experience: (1) that it is a construction from the data of experience, and (2) that it is a construction which each of us makes directly from the data of our own experience. When the phenomenalist's thesis is stated in this form it can be seen that it is (2) — what we may call the Special Thesis of phenomenalism, namely that things are constructions which we each make from our own experiential data, that has failed to stand up to critical examination. Can (1), what we may call the General Thesis of phenomenalism, that things are constructions from the data of (individual) experience without any stipulation as to how this construction is effected, give us any better account of knowledge? The specific cause of this failure of the Special Thesis of phenomenalism, it can now be seen, is that we were trying to show how the individual organizes his private data in such a way as to give rise to the (common) forms which he uses for the organization of his private data.

The difficulty has been frankly stated by Bertrand Russell:

> " . . . empiricism, as a theory of knowledge, is self-refuting. For, however it may be formulated, it must involve some general proposition about the dependence of knowledge upon experience; and any such proposition, if true, must have as a consequence that itself cannot be known. While, therefore, empiricism may be true, it cannot, if true, be known to be so."[2]

It is evidently the fact that experience necessarily means individual experience that has led to the phenomenalist's Special Thesis. There is a peculiar feature of this thesis which we have not yet taken into account. If each one of us constructs our world-picture, or what more generally we may call knowledge, from the data of our own experience, then knowledge — which is knowledge only if it is *common* and *communicable* —

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[2] *Inquiry into Meaning and Truth*, p. 165.

begins afresh with each one of us. But the implication of this is that each one of us is the source of *all* knowledge: the infant in his cot, starting from his state of original nescience, and confined within the limits of his own experience, is to be the originator of a body of knowledge which will have universal and necessary validity. All of which is, of course, absurd.

But this absurdity serves to draw our attention to the fact that the body of common forms we call knowledge, though analysis assures us that it necessarily has its beginnings in the data of individual experience, does not, as a matter of observable fact and independently of any theoretical considerations, begin with the experience of any one of us. The infant obviously does not generate the common forms out of his own experience; he enters progressively into an already established body of forms. Each generation takes over the forms (no doubt adding to them in the process) from the previous generation. This clearly means that the beginning of this system of forms is to be found, not in the present, but in the past, and this would necessarily mean, we must remind ourselves, not in present individual experience, but in past individual experience.

This is the statement of our problem. And at once it raises the question: Is the problem of knowledge to be solved by pushing it back from the present into the past? Superficially the answer might appear to be: No, the problem is not to be solved in this way. But on consideration it is seen that the answer must be: Yes, this is the only way in which it *can be* solved. It is only by following the problem back into the past that it can be solved, because it is in the past that it *has been* solved; it was in the past that men originally solved it.

We of the present generation are not setting out to solve the problem of knowledge. This is so obvious that it hardly needs to be stated. And yet, just because it is so obvious, it is easily overlooked. It is only because we have knowledge that the problem of knowledge arises for us. Clearly, then, the problem facing us is not, How *is* knowledge possible? but, How has knowledge *become* possible?.

The merit of phenomenalism is that it presents this problem to us in the specific form: How has common objective knowledge, valid for all experients, been derived from the subjective and private data of individual experience? And this, as phenomenalism also shows, is a question as to the *beginning* of knowledge. And this again must be taken as meaning the *very* beginning, the emergence of knowledge out of a primordial state of nescience. We are not talking about the beginning of knowledge, while we assume in any degree or form an already established body of knowledge.

III. *The Beginning of Knowledge*

At once, of course, the objection arises that we cannot know anything of such pre-cultural or proto-cultural beginnings that would be relevant to the solution of our problem. But here again phenomenalism indicates the answer, for it tells us, not what our primitive forebears thought — in any case we are discussing a state preceding thought — but what were the conditions out of which thought, and knowledge, and culture, necessarily arose. It tells us that these arose out of the data of individual experience. But it tells us more than this. For we know, from the failure of the Special Thesis of phenomenalism, that knowledge did not arise out of the data of any one individual's experience; a truth which seems to be independently confirmed in the "Egocentric Predicament", which draws our attention to the fact that the individual, by virtue of his individuality, is a closed unit of experience.

But it is at this point that the real significance of phenomenalism begins to emerge. As we have seen, knowledge itself is a refutation of the Egocentric Predicament. We *have* knowledge, we *do* communicate with one another. If someone calls out "Fire!" in a hotel corridor at night, everybody somehow escapes from his egocentric prison, and makes for the head of the stairs by the shortest possible route. This prison, in fact, is a purely theoretical one, inhabited only by philosophers, and by these only between nine and four, Mondays to Fridays.

But this does not mean that the Egocentric Predicament is a philosophical delusion. It does not apply to us, who possess knowledge, because knowledge, that meshwork of objective meanings through the mediation of which we break free from the prison of our innate subjectivity, *is* the transcending of that original psychic isolation; that, from the circumstances out of which knowledge has arisen is its essential and necessary character.

The truth of the Egocentric Predicament does not lie in the present, it lies in the past. Once knowledge is established, the "predicament" no longer applies, because knowledge is the overcoming of that predicament. But until knowledge is established, it does apply: each individual, as a separate experiential unit, is confined within the limits of his own private and incommunicable data. *It is out of the experiential data of separate individual men that knowledge is to emerge*: data which are discontinuous as between individual and individual, and indeterminate as not yet being determined within any common system of concepts.

In other words: *knowledge,* as the General Thesis of phenomenalism, backed up by the Egocentric Predicament, makes clear, *is a complex function of all men's experience.*

The steps that lead to this conclusion may be summarized as follows:

1. We *have* knowledge, which is a common inter-subjective system of meanings or forms.
2. It is innate in the human situation that the individual is confined within his own subjectivity — the so-called "Egocentric Predicament".
3. Knowledge, as an inter-subjective system of forms, is thus a transcending of these innate subjectivities.
4. Knowledge, as a transcending of these innate subjectivities, is thus a function *of* these subjectivities.

It will be noted that there is no reference here to *how* these innate subjectivities have been transcended. Our present concern is only with the fact — the fact that we have knowledge and that, because of the circumstances out of which it arose, this is its essential nature: the specific sense in which, as the General Thesis of phenomenalism asserts, it is a function of individual experience.

Knowledge is not derived directly by each one of us from the data of our own experience. It is an established system of common forms in terms of which we organize our private data. It is a historical growth, having its roots in our pre-cultural past, and its starting-points in the data of individual experience. Knowledge, as a historical growth, is a function of individual experience in the specific sense that it has been constituted out of elements of experience which are essentially discontinuous and indeterminate; it is the expression of a complex procedure of abstraction from, and correlation of, not a direct derivative of, these discontinuous and indeterminate elements of individual experience.

When knowledge finally emerges as a closely-integrated system of concepts and meanings, it is not the expression of a relationship between the individual and an external self-subsistent reality; it is the expression of a relationship between experiences, between the experiences of separate individuals. It is Man, not men, that knows. It seems natural to speak of the individual's mind "making a working model of reality",[3] but — leaving aside a difficulty regarding "mind" which is itself an abstraction — "reality", seen in the light of a rigorous empiricism, *is* a model which the individual uses for the interpretation of his private data. He does not construct the model from the data, he relates the data to the model.

The attempt to trace the steps from an individual's experience here and now to an "external thing" necessarily fails, because the individual is not "seeing" a "thing" in the sense assumed. The thing is a common experiential form, a function, by abstraction and correlation, of all men's experiences. The individual's act of "seeing" consists in subsuming his otherwise indeterminate immediate data under the appropriate common form. To "see" is to judge that these present data belong under that common form. When, to take a stock example, I "see" an elliptical brown patch and yet affirm this to be a penny, which is round, I am obviously not constructing the round penny from my elliptical datum. I am interpreting my datum in terms of the already constructed experiential form, penny. Seeing is the name we give to this judgment when it is carried out in terms of visual data.

But to say that knowledge is not a relationship between an individual and a self-subsistent reality, but a relationship between experiences, is not an assertion that "there is no such thing as objective reality". It is not an assertion as to either the existence or the non-existence of reality, it is an assertion as to how conceptions such as "existence" and "reality" have arisen out of experience. It says that in the nature of the case — in the nature of the fact that knowledge is a function of experience — the term "objective reality" means this experiential complex, which is objective in the sense that it is an abstraction, not from your subjective experience or mine, but from the experience of all individuals. Reality is the name for the essence of all experience; it is a piece of funded common experience.

That knowledge has a history going back into the dim recesses of man's past will hardly be questioned, but it might seem that this is not a fact having any relevance to the problem of knowledge in the complex form in which it presents itself to modern man. But it is not this complexity that constitutes the problem of knowledge. As a famous lady remarked, when the miracle was reported of a priest, who had recently been guillotined, being seen walking the streets of Paris carrying his head under his arm: "In such cases it is the first step that counts." In the case of knowledge, no matter what its final complexity, it is the first step, or, as we should now say, the first steps, that count; the first steps toward that complex procedure of abstraction and correlation — if something which was necessarily unconscious and groping and intuitive should be called a procedure — by which a structure of common, objective knowledge was to be built up out of the elements of private subjective experience; a structure which, because it transcended the experience of any individual, presented itself as being independent of all experience, and which, because it was valid for all experients, seemed like the self-subsistent cause *of* experience.

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[3] Quoted by E. O. Adrian *Physical Background of Perception*, p 93, from K. J. W. Craik's *The Nature of Explanation*.

It is the merit of phenomenalism — which is only empiricism carried to its logical limits — to have indicated where the solution of the problem of knowledge is to be sought, and what is the essential nature of the complex system of common forms which constitute that solution: common in the sense that they are a function of the whole diverse range of human capacities for experience; and forms in the sense that, as functions of all men's experience, they act as principles of organization for the experience of all individuals.

IV. *Implications*

It has not been found possible to account for the body of knowledge we actually possess, in terms of the individual's own experience. On the other hand, it can be seen to be equally impossible to account for knowledge as a function of experience, except as a function of all men's experience.

Between these two conceptions of knowledge as (*a*) a direct function of one individual's experience, and as (*b*) a complex function, by abstraction and correlation, of all men's experience, all human capacities for experience, there may not seem, at first glance, to be any very remarkable difference — until we note that the corpus of problems that has occupied philosophers over the past 2000 years has rested upon conception (*a*), the individual-experience view of knowledge.

From Plato's *Theaetetus* to Ayer's *The Problem of Knowledge* — "Surely I know for certain that these physical objects exist. And if I do know this for certain, I know it on the basis of my (past and present) sense-experiences" (p. 125) — and coming to a focus in the voluminous modern literature on sense-data, the whole development of Western thought has been based on the unquestioned assumption that what we call knowledge is a *direct* function of an individual's own experience. (The alternative view, that, as a historical growth, knowledge is a *complex* function of *all* individual's experience, has sometimes been glanced at but never systematically worked out.) But, on consideration, it is incorrect to say that the assumption has been unquestioned, because the whole development of Western thought has consisted in a progressive questioning and testing, and final falsification, of this traditional assumption.

Philosophers of a rationalistic tendency, assuming that the individual's own experience was not sufficient to account for the knowledge he possessed, have invoked some sort of super- or supra-experiential form or principle to supplement its deficiencies: Platonic Ideas, Leibnitzean pre-arranged harmony, Spinozan "the order of things the same as the order of ideas".

Those of an empiricist tendency, assuming that the individual's own experience was sufficient to account for the knowledge he possessed, set out to deduce this knowledge, either from experience itself, the unaided experience of an individual, or else from the nature of the self-subsistent object regarded as the cause of the individual's experience.

The empiricist assumed that the individual's experience *was* sufficient to account for knowledge; the rationalist assumed that it was *not*. Both assumed that the only experience the individual had available to him was his own personal and private data, and that it was out of these, either alone or backed up by some extra-experiential element, that knowledge was constituted. It is because the empiricists' undertaking, pursued with a sort of puritanical rigour, has been impossible from the start, that empiricism has proved in the outcome to be the more fruitful account of knowledge.

The rationalist could not account for knowledge, which is essentially a system of common forms, except by an elusive *petitio* in which he borrowed from knowledge the forms in terms of which he was to account for knowledge. The procedure worked as an account of knowledge, but not as accounting *for* knowledge. The empiricist in the long run could not deduce experience from the nature of the known object, because the object progressively revealed itself as essentially a function of experience: the known object was inescapably the object as known. But this left the empiricist with only experience, which he necessarily interpreted as, in fact simply assumed to be, the experience of an individual here and now. And the question that was implicit in this standpoint was: How was knowledge to be accounted for as a function of the experience of an individual having experience of that which was itself a function of his experience? Hume drew the line under this particular problem without having stated the explicit nature of the problem.

In this extremity, the attempt was naturally made to come to terms with the problem by keeping a foot in both camps, the object-world and the pure-experience world. This is in effect the solution attempted by Kant's great intellectual feat, in which the object became both a thing-in-itself and a function of experience, and knowledge a function of forms which dwelt, not in a Platonic heaven, but in the mind itself.

But the worm at the heart of empiricism was not to be so easily got rid of. The object-in-itself had to go — it could only be known as it was experienced — and only the individual's experience was left; and this proved itself too fragile and uncertain a basis for the complex and rambling structure of human knowledge. This was the merit of phenomenalism, that, as the final logical expression of empiricism, it brought this whole traditional effort of thought, both rationalist and empiricist, with all its great achievements, to a close. A pure rationalism, because of the *petitio* that lurked at its centre, would never have indicated in this way the end of one phase of thought and the beginning of another. For that is what the failure of traditional empiricism forces upon us: a new conception of our relationship as rational beings to ourselves and to that other which we call Reality.

We are not doomed, each one of us, on the basis of our own meagre experiential resources, to lay down our own crazy-pavement of inference from data to knowledge. Thought is much simpler, because so much more complex, than that. We live our lives, and do our thinking, whether as philosophers or as plain men, within an intricate texture of common meanings, the deposit left by all the past ages of human experience. I, as an individual, can know as much as I do because I am able to experience with all men's experiential capacities.

Sartre's tragic and typically twentieth-century cry: "My freedom is anguished at being the unfounded foundation of all value" is its own commentary on the effort to derive the whole of knowledge from the data of an individual's own experience. The effort necessarily fails, because knowledge is not a structure of that order, and with the failure the individual breaks under the strain of his own heroic presumption.[4]

In conclusion, it may be pointed out that two important questions have been raised in the foregoing discussion which require further elaboration. Reference has been made to the process of abstraction and correlation by which a common world-picture has been constructed from the data of physically and psychically isolated experiential units, from men of all kinds of sensory and affective make-up. The first question is as to how this procedure of abstraction and correlation has been carried out. The second question is as to how the product of this complex procedure, the resulting world-picture, has been made available, as a new and higher form of common experience, to all individual experients. In the first place the common picture is derived *from* the flux of individual experience. In the second place it is referred back, as an organizing principle, *to* the flux of individual experience. These questions require an answer before we have a complete account of knowledge as a function of experience.

North Tamborine, Queensland.

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[4] To anyone who feels himself tragically involved in this predicament this will not seem a satisfactory account of the matter, it will not relieve his sense of being "alone in the cavern of himself, listening to the soundlessness of inflowing fate". It is in fact only half the story. The other half I have sketched in "Philosophical Implications of the Modern Revolution of Thought" (*Philosophy and Phenomenological Research,* Sept., 1957) and also in "The Rational and the Real" (*Philosophy of Science,* July, 1957) and "Knowledge and Experience", (*Philosophy of Science,* October, 1957).

DISCUSSION

MR. BRADLEY AND THE LIBERTARIANS

By John King-Farlow

Mr. R. D. Bradley in two recent articles[1] of refreshing clarity and vigour has sought to remove old metaphysical puzzles about 'free will' and 'logical libertarianism' by taking a harder look at misunderstandings and misuses of words. Notably though he succeeds in uncovering confusion in the libertarian ranks, I shall argue that Bradley has still failed to look hard enough; that, when libertarians are purged of the muddles he rightly rejects, their views of freedom and the future may be reformulated in clear and defensible positions.

Bradley's approach to 'free will' can best be evaluated after we have examined his attack on 'logical libertarians'. By the latter title he means those who sympathise with Aristotle's refusal, in *De Interpretatione,* chapter nine, to apply the law of excluded middle to certain future indicative sentences. Bradley's alternative doctrine, 'logical determinism', exposes his own misunderstanding of how variously language can significantly function. According to his doctrine, "for any x and for any F, if F can significantly be predicated of x, Fx must be either true or false". (*Mind,* 1959, p. 193.) Every such Fx, it is held, must obey the laws of identity, non-contradiction and excluded middle. This doctrine can immediately be rejected as mistaken, since many indicative sentences of the subject-predicate form could not, though quite significant, be at all naturally called true or false or self-entailing; nor could they have a contradictory in the formal logician's sense.

What Austin has said of the performative character of indicative sentences like 'I promise . . . ' and 'I baptise . . . ' applies equally to many sentences of subject-predicate form without specific performative verbs. Gromyko might say when a vote is taken: "The Soviet Union is opposed to the admission of Texas and Tasmania to the U.N."; the judge might say: "John Christie, you will be hanged by the neck until dead"; the C.O.'s messenger might shout: "No. 2 Platoon is leaving in five minutes". In each case we can translate these significant predications as Fx, yet these represent not truth-valued propositions but categorically disparate expressions or *acts* of veto, condemnation and command. Thus all that Bradley is entitled to do with the law of excluded middle is to say, as do his libertarian opponents, "if *F* can significantly be predicated of *x* then it must be true or be false that '*Fx*' must be true or be false". The muddle-headed libertarian like Aristotle tries to justify his opting for "it is false that '*Fx*' must be true or be false" by talk about mere potentiality for being true or being false. But the clear-headed libertarian sympathiser like Ryle in *Dilemmas* gives hard-headed reasons for treating descriptions and predictions as categorically disparate, and these reasons Bradley fails to consider.

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[1] "Free Will: Problem or Pseudo-Problem?", this *Journal,* 1958, pp. 33-45. "Must the Future be what it is going to be?", *Mind,* 1959, pp. 193-208.

The real value of Bradley's attack on logical libertarianism lies not in his palpably false alternative doctrine but in his criticism of three dubious presuppositions held by some of his opponents. These are: "(1) that it strictly *makes sense* to talk about statements being true or false 'now' or 'already'; (2) that whenever we say that a statement about the future is true or false we mean that it is true or false now; (3) that if statements about the future were at the present time true or false the future would be unaltered by anything that anyone did, since, as Prior puts it, 'what is the case already has passed out of the realm of alternative possibilities'." (p. 199.)

Bradley exposes the desperate oddness of 'true or false now' in presupposition (1). He justly complains that in (3) — which like (2) depends on (1) — logical determinism has been equated with fatalism by strange talk about the truth of '*Fx* tomorrow' *making* *Fx* happen tomorrow. (pp. 200-05.) Yet Bradley's exposure of confusion among libertarians can be met by reformulation rather than surrender. As for presupposition (1), the weak-headed libertarians say "'*Fx* tomorrow' is neither true nor false now", but the clearheads should say: "'*Fx* on Tuesday' cannot appropriately be called 'true' now or 'false' now, on Monday." (The relation of *now* to the punctuation makes all the difference!) (2) becomes for the clearheads: "*If* we may now appropriately say that '*Fx* tomorrow' is true or false then we may now say that '*Fx* tomorrow' obeys the law of excluded middle." Likewise (3) becomes, not "The truth of '*Fx* tomorrow' makes tomorrow's events unalterable", but rather "We could only rightly call '*Fx* on Tuesday' a 'true or false statement' now on Monday if '*Fx* on Tuesday' were unalterably true or necessarily false".

These reformulations are not muddle-headed, though their originals were. They represent, not mystical descriptions of

The True and the False, but linguistic proposals for restricting the use of 'true' and 'false' as a subsidiary or partial means of enforcing a metaphysical picture of the universe. If 'metaphysical picture' conveys no apparent sense, take specific examples of pictures to which such proposals would relate. One might instance: Aristotle's contrasting picture of the realm of *ἀεὶ ἐνεργοῦντα*, about which all futures could rightly be called 'true or false' now, and the realm of moral man as undetermined *ἀρχὴ* among *τὰ ὡς ἐπὶ τὸ πολὺ*; Broad's mounting slag-heap view of events and his asymmetry of Space and Time; Kierkegaard's attack on the Hegelian picture of the world from the standpoint of Pure Being — an attack which implies that the spatio-temporal symmetry of quantification theory, urged against opponents by Mr. Bradley as by D. C. Williams and Quine, gives only a misleading God's eye view of man in Time. Williams has claimed that his symmetrical Space-Time merely provides a much needed 'lucent arena', neutral as to metaphysical questions about the openness of the future. However, the libertarian may hard-headedly choose to place his linguistic restriction on future 'truths' precisely because he wants his linguistic forms and conceptual scheme even for ordinary, non-metaphysical thought to reflect his metaphysic positively rather than to be neutral. There are further conceptual reasons why some libertarians fight especially shy of future 'truths' and cannot share Bradley's optimism about precognition. These I have discussed elsewhere.[2]

Now we may turn to Bradley's comments on 'free will' with a clearer appreciation of where he is likely to succeed and where to fail. He is certainly right to emphasise that "the reason why we must resist the temptation to give a straightforward answer to the free-will question is this: it is primarily not a question about what the facts are but rather about what we are to say of the facts" (*A.J.P.*, 1958, p. 34). He sensibly insists that the question of those who ask "whether anyone is *really* free", to the extent of having absolute "freedom from determination" in certain mental acts of volition, is not a "real" or "genuine" but a "pseudo"-question. For by this he means that no "actual or conceivable empirical evidence would settle it decisively". (p. 36.) He is admirably quick to turn his back on proffered disputes about predictability and quantum mechanics as being questions quite different from that of free will (p. 37-8.)

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[2] J King-Farlow, "Seafights without Tears", *Analysis*, 1958

But here Bradley's wisdom ends. He takes seriously only two radically opposed answers. Either the free will dispute must be dissolved by linguistic analysis, as Hume saw, since it "arises out of confusions about what certain words mean or . . . what their implications are" (This answer he supports for want of considering any other possibilities than the second, which he rejects.) Or there is the answer of C. A. Campbell, who claims that any serious co-operative person can settle the issue by the non-sensory empirical method of introspection in times of moral temptation; for "here", says Campbell, "the self is revealed to itself as a being capable of transcending its own 'formed character' . . . " (p. 39) Campbell holds in his following reply that Bradley has misunderstood him (46 ff.); even if this answer is fair, surely our hopes of settling any question by the introspective method are rather dimmed by the wide and widely divergent variety of inner goings on recorded by such inner explorers as Plato, Descartes, Spinoza, Bergson, Freud and Sartre. Some of these would call Campbell's appeal superficial and fallacious, others would call it superfluous, since they 'find' that consciousness can behold itself in *all* contexts, with due application, as pure freedom.

If, however, we agree with Bradley against Campbell's claim to introspective verification, we need by no means swallow Bradley's own bitter prescriptions. Although the free-will question is indeed one "about what we are to say of the facts", this does not mean we can always dispose of quarrels over "real freedom" as in all cases being the fruits of verbal muddle. With the muddle-headed, determinists or libertarians, this may be so. But if the libertarian is prepared to take a forthrightly metaphysical stand there is at least one excellent alternative. Bradley, as we saw in the case of his 'logical determinism', and as we see from his offering us only empirical solutions or verbal dissolutions for 'free will', is obsessed with verifiable true or false propositions to the neglect of other significant uses of indicative sentences. The metaphysician can hold that his talk about real freedom is not intended to describe new empirical facts but rather to prescribe how we should look at facts already familiar. Introspective verification of real freedom would be a misconceived endeavour for such a hard-headed libertarian metaphysics, which frankly aims at *getting us to regard* our mental acts in a certain way. Kant's account of Freedom as a regulative, not a constitutive, principle would furnish a good example of how a hard-headed libertarian can begin to reformulate the muddles Bradley so justly attacks — but Kant blotted his copybook by speeding on to a moral *proof* of noumenal realms.

Bradley is correct enough in saying that philosophers of free will go beyond the claims of common sense morality and use ordinary words in an odd 'real' sense. But his conclusion that therefore something is ripe for dissolution only points to his somewhat dated notions of significance. The growing interest of linguistic philosophers in metaphysical maps, pictures and symbols[3] suggests a way of going beyond Bradley's valuable but unduly pessimistic critique of libertarian problems. We may yet see a respectable alliance between rigour in analysis and traditional speculation.

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DO WE EVER VALIDATE MORAL STATEMENTS? A REJOINDER

By ROBERT HOFFMAN

Mr. Monro informs me that I erred in taking him to have made a general point about all reasoning, since he intended to make only a specific point about moral reasoning. Here, his is of course the authoritative statement. But if he did not wish to make such a point, I nevertheless do, because the distinction between inference rules and moral judgments does not seem to me to obviate argument along the lines I followed.

Let us take the example from Lewis Carroll. What I take Carroll to be saying is this: ((A & B) and (A & B entails Z)) together entail Z; but if we formalize this we have:

(I) $A \cdot B \,.\, A \cdot B \rightarrow Z :\rightarrow Z$

But what the Tortoise wishes to know is, granting that "A and B" is *true*, how one can prove the *truth* of "Z" if one does not also grant the *truth* of the hypothetical

(II) $A \cdot B \rightarrow Z$

What he wants to say is

(III) "$A \cdot B$" is true and "$A \cdot B \rightarrow Z$" is true; therefore "Z" is true,

and then to detach "Z" and assert it by itself. Achilles begins his "proof" of (III) by stating that the principle by which he detaches it is legitimate. But his statement is only a longer proposition from which (*a*) he still cannot detach "Z" and about which (*b*) the Tortoise can raise the same difficulty he has already raised. Thus, although Achilles sees that (III) is correct, his attempt to meet the Tortoise's demand for its formalization results only in longer and longer propositions from which "Z" still cannot be detached. Hence, as Mr. Monro emphasizes, *modus ponens* is stated as a rule of procedure and not as an axiom.

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[3] cf. contributions to D F. Pears' *The Nature of Metaphysics*, also R. W. Hepburn and Iris Murdoch on "Vision and Choice in Morality", *Aristotelian Society Proceedings, Suppl Vol.* XXX

But Mr. Monro neglects to give sufficient weight to the reason why this is so. What Carroll shows us is that the *assertion* of a proposition cannot be formalized. We may consider an assertion as a statement which represents an opinion actually consciously believed. Now although it is true that (III) is stated as a rule of procedure and not as an axiom, it is important to see that it is so stated because any attempt to *assert* it as a proposition believed involves us in an infinite regress. For, although adoption of a rule to put the sign "|—" before a proposition if that proposition is asserted need not concern us with whether what is asserted is in fact the case, we should be so concerned if the object of our attention were instead the *asserting* of the proposition. In the latter case, criticism would be directed by the Tortoise not at Achilles's view of the relations that obtain within the syntax of a formalized language, but at whether that which he asserts is true or false, i.e., whether his opinion with regard to the truth of the asserted proposition is sound or erroneous.[1]

Thus it is, that rather than burden ourselves with questions about the limits of knowledge by considering the statements of the Tortoise as assertions and, therefore, as epistemic in nature, we regard one of them, namely (III), as a rule of procedure. We then argue, quite rightly, that the justification of a rule of procedure is its systematic convenience and its existential serviceability.

Mr. Monro also points out that we are frequently confronted with people who doubt the soundness of our set of values and whose set of values we doubt, but that "this does not usually happen with induction". But my point does not depend upon its usually happening, nor even upon its happening at all; it depends upon the possibility of such a situation obtaining. As long as Mr. Monro does not deny the possibility of the state of affairs which I propounded, the argument I advanced is sound.

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[1] H. Jeffreys, *Scientific Inference*, 2nd ed. (Cambridge: The University Press, 1957), p. 186; A. N. Whitehead and B. Russell, *Principia Mathematica*, 2nd ed. (Cambridge: The University Press, 1925), I, 8-9; W. E. Johnson, *Logic* (Cambridge: The University Press, 1921), I, 3-3.

Elmont, New York.

MR. STOVE'S BLUNDERS

By J. W. N. WATKINS

With the wrong-headed aim of defending an inductivist view of empirical confirmation, Mr. D. Stove has, in paras. (*b*), (*c*) and (*d*) of his note,[1] ventured some even more wrong-headed criticisms of my criticism of that view.

Para. (*b*) consists of criticism of something I never dreamed of saying since it is inconsistent with what I did say.

This is more or less admitted at the beginning of para. (*c*), the rest of which shows that Mr. Stove just has not understood the force of what he rather peculiarly calls 'the well known paradox of the ravens'. He writes:

> For certainly no one would attempt to falsify "All ravens are black" by observing swans, shoes, etc., but neither would anyone attempt to instantially-confirm it by such observations.

But the 'paradox' arises precisely because swans, shoes, etc., *do* confirm 'All ravens are black' on an instantiation theory of confirmation (since they instantiate the logically equivalent hypothesis, 'Everything is no raven or black'). I am absolutely staggered that at this late date Mr. Stove can still say:

> It should have been obvious that what corresponds to "failure in attempts to falsify" is "success in attempts to instantially-confirm".

It should, on the contrary, have been obvious that, in at least ninety-nine cases out of a hundred, unsuccessful falsification and successful instantiation are *not* equivalent. If I merely wish to *instantiate* 'Everything is no raven or black' I shall have succeeded if I search for, and find, a number of objects which are not ravens or black. But since I can very well search for non-ravens and black things *without* also searching for non-black ravens, my successful instantiation will not be equivalent to an unsuccessful attempt at falsification.

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[1] "Popperian Confirmation and the Paradox of the Ravens", this *Journal*, August, 1959.

In para. (*d*), basing himself on a remark which he attributes to me (it was actually an explicit quotation from Professor W. B. Gallie's *Peirce and Pragmatism* which I had used in a quite different connection), Mr. Stove attributes to me a sort of Labour Theory of confirmation:

> The degree to which an hypothesis is accredited depends on the amount of effort that has been expended upon it.

A little more effort on his part would, I must concede, not have been out of place. He speaks in his title of 'Popperian Confirmation'. Had he consulted the article by Professor K. R. Popper[2] to which I referred, he would have found that on a Popperian theory of confirmation 'the degree to which an hypothesis is accredited' by an observation varies, not with the observer's perspiration, but with: (i) the ***improbability*** of this observation given existing knowledge *without* the hypothesis in question; and (ii) its *probability* given existing knowledge *plus* the hypothesis. This is an objective measure and has nothing to do with 'the history and psychology of individual scientists'.

None of Mr. Stove's criticisms has any validity whatever.

London School of Economics.

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[2] "Degree of Confirmation", now reprinted as part of Appendix xix of *The Logic of Scientific Discovery*, London, 1959, p 395-402

CRITICAL NOTICE

On Selfhood and Godhood. By C. A. Campbell. (Gifford Lectures, 1953-4 and 1954-5, at the University of St. Andrews.) London, Allen & Unwin, 1957. 35s. (U.K.)

Contrary to recent precedent, Professor Campbell has brought together his two courses of Gifford Lectures in a single volume. The result is 433 pages of consecutive argument; a feat for which, in these days of discontinuous snippets, he deserves our warmest congratulations. Once again, we have been shown how the Gifford Foundation helps a philosopher to perform his traditional office of gathering up the threads of a lifetime and knitting them into an ordered pattern.

The main themes of the book may be stated briefly. In the first part, Campbell exerts himself to defend the notions of "self-activity" and "free-will", understood, I am glad to say, in the obvious and vulgar sense. In the latter, distinguishing between "rational" and "supra-rational" Theism, he finds the former rationally indefensible, and is cautiously prepared to endorse the latter, relying heavily for this purpose on F. H. Bradley's conception of a supra-relational Absolute.

It is part of the author's contention that "it is impossible to discuss the deeper problems of religion save on the basis of a carefully considered theory about the essential nature of the human self" (Preface, p. xi); and it is this conviction which should form the link between the two courses of lectures. As the link, unfortunately, turns out not to be as close as was anticipated, a few preliminary observations on "selfhood" and "Godhood" may perhaps be in order; and it will be convenient to connect them with Campbell's lifelong interest in the work of F. H. Bradley.

In general, it is by no means obvious that a philosopher solicitous of doing justice to religion should emphasize "self-activity" as Campbell does; and free-will understood as Campbell understands it is quite incompatible with some widely accepted religious assumptions. In many Eastern religions the first duty of man is to seek to be absorbed or annihilated. Even within the Semitic-Western tradition, the separateness of souls has been denied (by Averroes, for example, when he asserted the "unicity of the active intellect"); and even among those who have asserted the separateness of souls, it has usually been held to be irreligious to defend free-will: not only among orthodox

Moslems and sectarian Calvinists, but also, by implication, among middle-of-the-road theologians who carelessly allow themselves to use the noble adjective "Pelagian" as a term of abuse. It cannot therefore be said that without "self-activity" and "free-will" there can be no "religious consciousness". At the same time it can be argued that a religion which accepts and dedicates the personal qualities is better, as religion, than one which rejects and tries to discard them. Much of Campbell's early argument is to this effect, and it is greatly to be welcomed.

But the defence of this thesis, and consequently the adjustment of the two lecture courses, is embarrassed by Campbell's frequent resort to F. H. Bradley. For even Hume and the Hinayana Buddhists have dealt no more destructively with the notion of "self" than did Bradley in the famous chapters ix and x of *Appearance and Reality;* and as for free-will, it does not even appear in the index to that work, though it is referred to (in a foot-note, p. 435) as a "lingering chimaera", which "no writer who respects himself can be called on any longer to take seriously". It is clear that in the first series of lectures Campbell is taking up a position which his main witness in the second series of lectures would have regarded both as wrong in itself and inconsistent with their common philosophical premisses. There thus arises a certain uneasiness lest the two lecture-courses should fail to coalesce. How far that uneasiness is justified can only be discovered by a study of the detail.

It is not proposed to discuss the first set of lectures at great length; not because what is said there is not important, but because the reviewer is so thoroughly in agreement that there is little left for him to do but to applaud from the grand-stand. To maintain at least the relative unity of the subject in relation to its objects; to insist that the unity of the subject cannot be translated into the unity of an object; to defend the evidence of introspection as the way in which we are aware of ourselves as subjects; to vindicate the notion of "self-activity", or as I have been accustomed to put it, initiative, against resolution into a jostle of separate mental and/or physical events; to state the case for free-will without ambiguity or compromise; and to show that the "experience of selfhood in its moral mode" is altogether different from the experience of emotional compulsions or concealed imperatives: all these are endeavours which are as pertinent as they are uncommon, and they will warm the cockles of any honest reactionary's heart Studied in detail, they will do more good to head and heart than anything published on the subject for a long time. After a dismal period of apologizing to strange gods in outlandish idioms, the soul has once again assumed the offensive.

Leaving the general discussion of these themes, then, to the "progressives", who, on past form, should have plenty to say. I propose to make only three brief and particular observations, and then to pass on. The first is that Campbell's argument is interwoven with idealist reminiscences which are not essential to it, and could deter non-idealists, needlessly, from accepting it. He holds (p. 39) that the centrality of judgment in human cognition, and the centrality of self-consciousness in human experience generally, are at any rate the most persuasive considerations in defence of personal identity. But those who believe that knowledge is in the end awareness know how hard and continuously one has to work to achieve that awareness, and those who believe that the self is disclosed primarily in the cognisance of objects are just as well placed to argue for its permanence and integrity as those who find it necessary to duplicate consciousness into self-consciousness. The argument can, but need not be, conducted on Idealist lines: it is not the Idealism which is crucial, but the incidental emphasis, which not all Idealists share, on moral effort and freedom of will.

This leads to the second observation. Campbell asserts that moral praise and blame stands or falls with "the categorical proposition that X could have acted otherwise because — not if — he had chosen otherwise" (p. 164). The reviewer notes his obduracy with peculiar satisfaction, and recalls with pleasure that he and Campbell independently delivered inaugural lectures in defence of the vulgarest possible conception of free-will, longer ago than either may care to remember. In particular, he welcomes the distinction between the self as forming and the self as formed (p. 177) and the use that is made of it to outflank the fashionable contention that an act which does not flow from a man's formed character does not flow freely. Indeed one might go further, and say that the act of forming his character is just as much a continuation of the character of a moral agent as the character which has been formed. Thus expressed, the libertarian doctrine can admit and appropriate the objection that free decisions cannot come from nowhere.

The third observation is a short one. It is that Campbell does not waste time trying to demonstrate that the problem of free will is not a pseudo-problem. Not only has he already dealt with this issue at length (*Mind*, 1952), but it should be a point of policy in a straight work on metaphysics to barge ahead without stopping to brush flies off one's back. It is time to turn and swat them if they really sting.

The rest of this review is concerned with Campbell's account of religion; and this, as indicated above, is more debatable. The central question is whether the defence of religion by way of free will is consistent with the defence of religion by way of Bradley. It is only in the last chapter, however, that the vital role to be played by Bradley is fully disclosed; and there are earlier themes which invite examination. Campbell moves back to Bradley over a series of failures; and if we find that in the assay of religion Bradley is a false friend, we shall want to be sure that the alternatives are really failures.

The first point comes out already in the "Prolegomena", where it is stoutly affirmed (p. 32) that "the only faith that is fitting in a rational being is a faith that is buttressed by reason". "The task remains for reason to discriminate, with the highest measure of probability that the case allows, between the authentic and the spurious among the ostensible revelations" (p. 22). Elsewhere, less happily, but more than once, he asks, "Is religion true?" (pp. 5, 421). It could be asked whether "being true" is all that men expect from religion; certainly there are codes and ceremonies, to say nothing of the central activity of worship, to which "true" and "false" are not the appropriate terms to apply. But no religion is wholly separable from its beliefs, and to these "true" and "false" are certainly appropriate. It is, then, entirely proper that religion, at this focal point of belief, should be made the subject of rational investigation. The move to claim exemption for it, though it fits very well with the recent attempts of philosophers to destroy their own credentials, is one which metaphysicians like Campbell (and his reviewer) are surely required to clamp down on.

At the same time, metaphysical statement is not a *substitute* for religious belief. Religious belief is always expressed in particular and historical terms: e.g., "The word was made flesh, and dwelt amongst us". Metaphysical statement, on the other hand, is highly generalized and necessarily oblivious of specific affections. No amount of metaphysics could *produce* religious belief, if it was not, so to speak, there already. On the other hand all religious belief assumes a metaphysical alignment "The word was made flesh" is incompatible (*a*) with simple materialism and (*b*) with *a priori* mind-matter dualism. And at that point two policies are feasible. We can either say "The word was made flesh, and therefore materialism and dualism are metaphysical mistakes"; or we can say (after full metaphysical discussion) "Materialism and dualism are metaphysical mistakes, therefore we are entitled to say 'the word was made flesh' ". The average believer, if he faces the issue, is inclined to take the first road. The metaphysician, however, with some reluctance if he is also a believer, just has to take the second.

On this issue Campbell is firmly on the rationalist side. He has no doubt that faith *needs* to be "buttressed" by reason, and he is not at all prepared to accept revelation *as* a reason. The philosopher, therefore, has a vital part to play in the defence of Christianity, and he can play it properly only if, *qua* philosopher, he is uncommitted. He is like the private detective who undertakes a case with the words "I accept it on the understanding that you want the truth, whatever it may be". This is not to say that philosophy *is* religion at a higher level, in the manner of Hegel, *and of Bradley;* but it is to say that religion *needs* philosophy.

The second phase of Campbell's argument concerns the concept of religion. As he notes, there are many religions; and it is most unsatisfactory to reduce any of them to what is common to all of them, for what produces in various cults the peculiarly religious attitude of worship is not always what is common to all of them, and most frequently what is peculiar to each of them He starts, soundly enough, with the attitude of worship, and proceeds to say that "religion involves belief in the existence of a being of supposedly worshipful character" (p. 237): i.e., of one endowed with transcendent worth and transcendent power (p. 245). But he notes that in the so-called "apotropaic" religions, which are mainly concerned with the devout placation of devils, there is an overwhelming sense of power and little sense of worth, and these he calls, bluntly, bad religions. This means that there must be a standard for religions, and not merely an average, i.e. they are to be measured by their best. It is along these lines that he works forwards from religion to theism: "Theism is not just one species of religion amongst others, but rather the proper culmination of the development that is intrinsic to religion as such" (p. 255). The sense of worship can be attached only to that which is, at least supposedly, perfect: and this is a criterion which only the traditional attributes, viz., unity, eternity and, in some sense, omnipotence, are able to satisfy. It is to be noted in passing, in the interests of what follows, that at a crucial point in the discussion (p. 264), Campbell swings with apparent indifference between "imperfect" and "self-contradictory", i.e. between religious considerations and Idealist-logical considerations. For the moment, however, the conclusion is that "the logical nisus of the religious consciousness presses forward to a theistic consummation" (p. 268).

The third and most distinctive phase of the argument concerns the sense in which the traditional attributes of God are to be understood. Are we to take them literally or symbolically?

It is important to be clear just what this question means. It could be intended just to ward off the popular misconceptions caricatured in Blake's Old Nobodaddy, which no reflective theologian has taken seriously. Or it could be by way of insisting that when God is said to walk in the garden in the cool of the evening, He does it very differently from a commuting stockbroker: which, again, no reflective theologian would dream of denying But Campbell carries it much further than that. What he attacks as "literal" is what he calls "rational theism", which asserts in God the attributes of power, wisdom and goodness as "identical in principle with those qualities as we know them in human experience" (p. 270) The difficult words are the words "in principle". It would be generally agreed that attributes such as power, wisdom and goodness are not applicable univocally to God and man But the danger is that in reaction against "literalism" they may become so attenuated as to stand for nothing at all except human desires and aspirations. This was the ground for the condemnation of "symbolism" in the Encyclical *Pascendi* in 1907.

The difficulties Campbell has in mind are illustrated in the classical notions of "divine intellect" and "divine will". Both intellect and will, as ordinary human qualities, are necessarily imperfect, and therefore not only self-contradictory, but unqualified to accept the tribute of worship. But this is not merely because they are human: it is because they are will and intellect. *Any* will, *any* intellect, must necessarily be imperfect, because it is of the nature of both to seek completion in something other than themselves This is as true of "totalistic" thinking as it is of discursive thinking; as true of the unchanging will revealed in natural law as of the will directed to objectives varying from day to day. "No conation without imperfection": and, as judgment, intellect is (though Campbell does not get round to saying so) a kind of conation Thus will and intellect are intrinsically imperfect, and cannot be properly attributed to God, the object of worship.

It must here be said plainly: if this is true, then the only conclusion is an utter agnosticism. There is *no* feature of the known world which can give us *any* inkling of the divine nature. We can only, if we will, go on worshipping we know not what. Campbell's own conclusion is less radical: "Either symbolic theology or no theology at all" (p. 323). But how can there be symbols when, by hypothesis, no symbol will serve? If what Campbell says in chapter xv is true, the foundations for a Symbolic theology are taken away. It is true that later he replaces them, stone by stone, till they take on a quite impressive air: but that is because he surreptitiously goes back on chapter xv. This will be argued later; our concern here, on the assumption that the unqualified acceptance of chapter xv would wreck the whole enterprise, is to save the enterprise by introducing qualifications.

If we ask *why* thought and will cannot belong to a perfect being the answer is just that they do not terminate in themselves but lead on to something else. This might be an objection if what it led on to were not a further excellence; but in the advance from excellence to excellence, where is the imperfection? Obviously, in the advance: what acquires a new excellence cannot have been absolutely excellent before it acquired it. Here at last is the problem; as so often, it is a problem about time.

The whole theological tradition, descending from Plato and Aristotle, and effectively smothering the Christian witness to the contrary, it that the perfect must be perfect once for all. On the assumption that things are getting worse (i.e., on the Greek pagan assumption) this conviction is understandable. but if we hold in any sense at all to the doctrine of the Holy Spirit, if we believe, with the pastor who said Godspeed to the Pilgrim Fathers, that "the Lord hath yet more light and truth to break forth from His word", we must also frame a picture of God as passing in time from perfection to perfection, being at all times perfect, perfect now and perfect then, but perfect then and now in relation to the world then and now. So conceived, God is not transcendent immobility, but transcendent activity, moving in time as its master, perfect at each successive moment, but not, in the old phrase, *simul et semel*. And in this there is no derogation of perfection: never to lapse from perfection in all possible time is just as good as having no time to lapse in. Indeed, from the point of view of worship, it is very much better: for the God who can neither think nor will, impassive and unresponsive, incapable of dialogue and inaccessible to prayer, does not answer the requirement that the object of religion should be worshipful, and the frostbound negatives which clamp Him in are not honorific, but genuinely privations.

In this case Campbell allows himself to follow the tradition too closely. He takes for granted the timelessness of the one God — missing, as I think, what Bradley comprehended, that the one God who is truly timeless ceases to be God and becomes merely One. He never questions the dogma of eternity. It is doubtless not his business to reflect that the conception of "everlastingness" is more Biblical, but it is his business to meet the objection that it embodies a rival and, from the religious standpoint, a preferable view of perfection. For, if the objection were valid, "thought" and "will" would no longer be as inapposite in their application to God as, following Bradley and the Neoplatonists, he too easily assumes.

So far we have been trying to save "rational theism" from "supra-rationalist" criticism. We have now to show that, if this is not done, the "supra-rationalist" theism to which Campbell concludes cannot even get started. We shall proceed by watching the stages of his argument, and we shall find that he gets through only by borrowing back from the estate which he has repudiated.

The first stage of the argument is that supra-rationalist theism is congenial to the religious consciousness. To this end he cites Otto, and with great effect. The "numinous" is not a rational category, and in all religion there is an element of mystery. So much the most rationalist of "rational theists" will readily admit, and if Campbell thinks otherwise, he is belabouring a man of straw. But Otto insists that between the numinous and the world there is a certain continuity. This comes out particularly in his description of the religious response — after all, it is the phenomenology of religion, and not primarily theology, which is the subject of his study. Unlike the more downright opponents of rational religion who have called Otto into their service,[1] Campbell is alive to this feature of Otto's thinking. "The numinous emotions . . . have their counterparts in ordinary life, and we can recognize the analogy between them, but in their numinous context the emotions are raised, as it were, to a new dimension" (p. 331). But, in that case, the numinous (or supra-rational) is in some respects (admittedly not in others) *similar* to the "counterparts", and the human attributes of thought and will do not need to be discarded, but rather to be sublimated, in the divine mystery. It is thus that Campbell proceeds to his "symbolic" theology. He is entirely justified in so doing; we should merely wish to remind him that to that end he must resume something of what he has discarded.[2]

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[1] e.g., T. McPherson in *New Essays in Philosophical Theology*, ed. Flew and MacIntyre

The second stage of the argument is that "supra-rational theism is valid[3] as the theoretical expression of religion" (p. 360). To establish this point, Campbell has to face the difficulty of how to say anything at all about what, being beyond reason, is strictly unsayable. He meets it by means of a symbolic theory of religious truth which is clearly the pivot of his whole theory. The "rational concepts" which are not applicable to God "in their literal significance" are applicable "when understood not as literal portrayals, but as appropriate symbols of the Divine Nature" (p. 345). Applied to the problems of chapter xv, this would presumably mean that to speak of God as mind or will is more "appropriate" than to speak of Him as nescience or automatism; and it is tempting to ask why. Such temptations, however, must be resisted till we have inquired what Campbell means by a symbol. We shall see that he escapes the difficulty and prepares for a further advance by means of an unwitting retraction.

Symbols may be either conventional or natural. The characters attributed to God are not conventional; i.e., they are not the product of a common agreement. Therefore they are natural; i.e., the symbol is suggested by something in that which is to be symbolized. Again, symbols are either arbitrary or analogical; and the characters attributed to God are not arbitrary, i.e., they could not be changed at will, like the Arabic 10 and the Roman X. They are therefore analogical; i.e., there is something in common, as well as something different, between the symbol and that which is to be symbolized. Indeed, Campbell is especially concerned to clear the symbol of any trace of subjectivity: "In order to be able to accept it as a valid symbol of God, religion would rightly insist that its identity (sic) with

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[2] Much of the trouble, I think, is due to Campbell's equation of "rational theism" with "literal theism" (e g. p. 340 "rational theism which applies its predicates to God in their literal meaning"). And this raises the question, who *are* the rational theists? Surely not the orthodox theologians, who talk, like Hume's Demea, about "adorable mysteriousness", nor the scholastic philosophers, who claim to prove *that* God is but not to know *what* He is, nor the writers of the Gospels, with their penchant for parable, nor yet the mystics, who report, "I have seen", but know that language cannot encompass what they have seen. There are left only those who mistake poetical characterization for literal description. and it is hard to see why these should be called "rational"

[3] "Valid", he adds, "in the sense that we have in it the logical *terminus ad quem* of reflection upon the object of religious experience and its implications"

God be shown to be not a subjective identity . . . but an identity that is in some intelligible sense *objective* or *necessary*."[1] Admittedly at this stage he is discussing the proper "theoretical expression of religion" (p. 360), and not the question, which he holds in reserve, "Is religion true?". But, even so, the word "analogical" strongly suggests the Thomist "analogy of being": and in insisting that the characters of God are not merely attributed to Him but are in reality in respect of His infinite being what the same characters in us are in respect of our finite being, he is siding with the realist tendencies in Thomism against the agnostic.[2]

But to suppose the matter to be thus settled would be to under-estimate the capacity of Idealists for turning things into their opposites. The propriety of the symbol, after all, is determined "by the very constitution of the human mind", not "subjectively", of course, "by anyone's personal choice or private history", but by the human mind as such. This determination Campbell calls "objective", and it appears to be the only meaning that "objective" has for him. The attributes of God are *imputed,* then, *by us,* to a being in Himself unknown.[3] At the best we are taken back to the "analogy of attribution", with its leaning to a sanctified agnosticism; at the worst, God is merely a magnifying mirror for the "inward necessity" of man. And such a view is offered as a 'theoretical expression of religion"! It is sufficient answer that no believer would touch it.

It is possible, however, that this diversion of the religious consciousness into Idealism may be overcome in a higher synthesis: with Idealists, one never knows. We must therefore proceed to the third stage of the argument, in which Campbell develops a view of the "general nature of ultimate reality" (p. 383) and tries to show how it "corresponds" (p. 404) to his account of what is theoretically required in religion.

We need not loiter over Campbell's metaphysical vistas: they are, and are acknowledged to be, substantially F. H.

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[1] p 353: the italics are author's; the interpolated "sic" is the reviewer's, and expresses his surprise

[2] In Appendix D. Campbell notes the famous distinction between the analogy of attribution and the analogy of proportionality, as conveniently presented to him in the writings of Dr. E. L. Mascall He is there concerned to argue *ad hominem* that the doctrine of analogy, even in the latter and less agnostic form, is at least as agnostic as his own doctrine of Symbolism As he observes, the matter requires detailed discussion which cannot be undertaken in an appendix But neo-scholastic discussions of this sort of problem are *the* standard discussions, and should not be left to an appendix.

[3] cf. p. 402: "The religious mind is impelled to interpret its object *symbolically* in terms of the highest conceivable power and value; and because this affinity has nothing subjective or arbitrary about it, but is based on 'an inward necessity of the mind' this symbolism . . . has objective validity as symbolism." cf. also p. 432.

Bradley's; with the supra-relational features of the model set in relief, and the distinction between the Absolute and the relational universe of discourse sharply enforced. What we have specially to note is how the factors thus distinguished stand to one another, as they might well provide a paradigm for the relation between a God of religion and the phenomenal world. The relation is that thought, carried to its limit, condemns its own separateness from its object, and that the criteria by which it works can be satisfied only if that separateness is transcended. Now in this instance the notion of system is common to thought and to reality, and while it is as thought that we are directly acquainted with it, it is displayed pre-eminently just at the point at which we cannot follow it. In this way, discursive thought pre-figures the supra-rational whole, and the supra-rational whole is properly symbolized by concepts drawn from discursive thought.

Here we are asked to note (p. 403) a "highly significant parallel" between "the religious view of the ultimate reality it calls God", and "the philosophical view of ultimate reality". Both proceed by the symbolic description of being through attributes drawn from human experience, concerning the propriety of which we know only that they are imputed not arbitrarily, but according to "an inward necessity of the mind". The parallel certainly has its interest, and, if pressed, should provide at least an Idealist-type correction of a too agnostic interpretation of religious knowledge. But the question which continues to worry us is whether it is really a parallel. To compare religious knowledge with philosophical knowledge as things independent is consistent neither with religion, which claims supremacy, nor with Bradley's Idealism, which also claims supremacy, nor with Campbell's own contribution, which alternates oddly between religion and Bradley, but nowhere suggests a division of territory. On the whole, his view is Bradley's: religion is the next best thing to metaphysics, and the appropriate metaphor is not "parallel" but "pyramid". Given the conception of metaphysics which Campbell shares with Bradley, only Bradley's conclusion is in fact logically admissible. In his peroration, however (421) Campbell contends that "straight metaphysical thinking" (as opposed, presumably, to the search for the presuppositions of religion) "leads independently to belief in an eternal being who is the sole ultimate reality". The incriminating indefinite article gives the full sanction of philosophy to what Bradley would have regarded as the weakness of religion, that its God is not *the* One, but *a* One who is master of the many. The change might well be thought an improvement if it had been intentional: but it is slipped in so casually that one can only suppose that the author does not see the importance of a distinction which is nevertheless highly relevant to his whole position.

Let us now attempt to evaluate Campbell's answer to his own question, "Is religion true?".

(1) He is clearly right in insisting on the intellectual component of religion; and he is further right in insisting that anyone who makes assertions has to be ready to support them by reasons.

(2) He is justified in raising the problem of symbolism in religion, and his refutation of crude literalism in discourse on deity is effective. But, as he himself points out, there are appropriate and inappropriate symbols, and unless there is some knowledge of God, as opposed to knowledge of ideas about God, it is impossible to determine which is which. If knowledge of God were *only* symbolic it could not even be *symbolic*.

(3) Bradley's metaphysic culminates in a concept of the Absolute which is distantly analogous to discursive thought and can be reached only by discursive thought as it discovers its own limitations. In the case of religious concepts, Campbell is inclined to be more exacting; he does not, for example, present the concepts of divine reason and divine will as stepping-stones to understanding, but even as obstacles. We have seen that a symbolic theory can be judged adequate or inadequate only if we already know at least what it is trying to symbolize. From that point of view, the analogy of Bradley's metaphysics is instructive. But most instructive of all, perhaps, is Campbell's own observation on propositions affirming the metaphysical concepts of unity, infinity and eternity. On p. 406 he writes: "What I suggest is that these propositions are literally true in respect of what they deny, but only symbolically true in respect of what they affirm." The same could be said of propositions referring to the mind and will of God; and, once this is admitted, there is no need to turn one's back on these concepts in order to provide an intellectual explanation of religion. Indeed, they might even prove to be the best possible symbols, not only for the religious, but also for the philosophical consciousness, and in that case, by holding them firmly in place, we might save religion from the cannibal distinction of being swallowed by philosophy. To discuss these issues would, unfortunately, take us too far afield.

In the course of a detailed review of a thesis with which he is not wholly in sympathy, it is difficult for the reviewer not to look more of a caviller than he feels. There is in fact much to admire: the firm emphasis on freedom and decision; the tough-minded (perhaps too tough-minded) debunking of the so-called "problem of evil"; the invigorating avoidance throughout of anything approaching sentimentality; and, above all, the strongly marked personal imprint which leaves nothing except Bradley quite as it found it. For all these qualities, and for many others, the work is important reading, particularly for those of us who will not at any price let either philosophy or religion go. But it must be added that at least one of those so situated finds Campbell's accommodation disappointing; all the more so as he is convinced that, if Campbell's original programme had been adhered to, the cause of the disappointment would never have arisen.

He set out with the firm conviction that a clarification and defence of the notion of "self" was essential to a philosophy of religion. While admitting that there are religions which play down the "self" as thoroughly as Bradley does, I cannot but agree with Campbell that they do not provide the proper scope and encouragement for a positive morality, and therefore find them inadequate expressions of a man's whole being. But, just when the affirmations of the first series promised a strongly personalist approach to religion, and thereafter to philosophy, an excessive reaction against popular anthropomorphism and an increasing dependence on Bradley prevents him from attempting it. The personal characteristics of God are reduced to symbolic representations of we know not what, and the most that Campbell will say for them is that they are less misleading than their opposites. It may be argued that on the "phenomenal" level there is room for the free will which is gathered up with all its uniqueness of flavour into the absolute: but, if so, it is a low-level phenomenon, and it cannot be in any sense an attribute of God. Bradley denies it *in toto* and I believe Bradley is consistent. It is noteworthy that the traditional attributes Campbell is most eager to reinstate are those of unity, infinity and eternity (e.g., p. 405): exactly those which most completely remove deity from humanity. And to the end (p. 411) those which he is least prepared to concede are thought and will. It is evident that Campbell's God resembles his Absolute at least in this: that he is objective system at the expense of personal being. It is unfortunate that he prefers Bradley to his own intimations of freedom. It is certain that he cannot have both. It seems a lot to ask, but it would be fascinating to see him start again, unimpeded, from his own intimations of freedom.

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REVIEWS

MY PHILOSOPHY AND OTHER ESSAYS ON THE MORAL AND POLITICAL PROBLEMS OF OUR TIME. By B. Croce. London: G. Allen & Unwin, 1949. 240 p. 15s. (U.K.)

The essays in this collection (selected by R. Klibansky and translated by E. F. Carritt) are not all dated, but, of those which are dated, most are from the 40's and the general impression is of work of Croce's later years, though following the main lines of his earlier work. Croce, of course, considered that philosophy could be worked out only in connection with concrete problems of the major departments of human culture; nevertheless, the presentation of separate essays on questions of history, politics, ethics and aesthetics gives an effect of disconnectedness and incomplete development of points and throws into relief the *weaknesses* of his doctrines.

The first essay, "My Philosophy", shows that what repelled Croce in Hegel's dialectic was what it had of formal rigour, and the decision, to which he found himself forced, to replace Hegel's idea of a "final system" by the idea "of a provisional dynamic system constantly developing and of provisional and dynamic systematisation" (p. 20) is a playing fast and loose with the notion of "system" or of philosophy. This is borne out in what is said in the note (p. 224) on "The Final Philosophy", a conception which is rejected because any philosophy is a step in the historical development of philosophy. "No doubt there is something 'final' or 'perennial' in all philosophies, but it is not strictly their philosophy, rather it is the unchanging subject-matter of all philosophies. This is self-consciousness, which is only active in the perpetual posing and solution of particular problems, an activity it could not perform if it were not essentially one and unchanging in its fundamental and eternal categories." It is impossible, nevertheless, "to pick out these categories and to establish them above, and in abstraction from, the particular philosophical context with which they are not so much combined as fused . . . . Every such attempt to demonstrate and define final concepts reveals, on due analysis, the historical context which has contributed to its result and which makes it a determinate philosophical theory *hic et nunc,* sufficient for the day . . . but not sufficient for the morrow, when it must be refreshed and reformed." If this means anything it means that what is "perennial" in philosophy (or anything that would enable us to say that this and not that is a philosophical question or is a piece of philosophical work) *cannot be stated*. Either "self-consciousness" is also a concept relative to a historical context and no more fitted than any other to provide a permanent subject-matter or it is bereft of content and can never be shown to *generate* concepts or to "fuse" with any set of particular problems. What the translator, drawing on Bradley, renders (p. 15) as "a wearisome 'ballet of bloodless categories'" is just what gives positive character to Hegel's doctrine (gives something that could remain as the object of philosophical theory even if the empty "self-consciousness", supposed to generate it, were dropped), and, when Croce excises this, he can (cf. p. 227) call anything or anyone (even "very unpretending people" who have no formal theory) "philosophical" — but neither "getting clear ideas" nor being "in the possession of the substantial truths" gives us any clear or substantial notion of what it signifies.

The same emptiness is seen on the moral side (in the essay entitled "The Identity of Philosophy and the Moral Life") when Croce says that those "who are called eminently men of character feel the inter-connection of all spheres of action, and that they cannot be truly or sincerely virtuous in one unless in all, since all depend on the one principle of conscience" (p. 229). And the same amalgamation of distinct questions is seen in the essay "In Praise of Individuality", when he says that there "is no reality outside the passion which we feel, the truth we know, the act we will; all is rounded within this circle of the spirit". And "true progress is our own progress, the progress of the world in and through us, which is always going on and so is without end. We all feel this progress in every good or useful action, in every new truth, in every experience of beauty; and in feeling it we rejoice that the world is alive and going forward in spite of trouble, ruins or disasters" (p. 206). Leaving aside the conventional and uncritical character of much of this, we might agree that there is a special sense in which all goods are progressive (it is certainly an important question for ethical discussion), but to say so does not in the least imply that everything spiritual (everything "within us") is progressive or again that everything real is spiritual, that we inhabit a "spiritual world". Also, it might be admitted that it is especially from cultural or historical problems that philosophical thinking comes, that it is the "difficulties" of practical life that stimulate our discovery of formal distinctions and connections — and that it is for this reason that philosophy, treated as one "specialism" among others, is so barren. But this would give no ground for saying of philosophy and history that "the whole of one is the whole of the other", for taking all problems as having a human *content* and not distinguishing from special contents the *forms* which are to be found in human and non-human material alike.

Croce's weakness on the formal side is illustrated in his treatment of classification, which he describes (p. 199) as having an instrumental and not a cognitive value, and takes to be misunderstood by the childish imagination of untrained minds which "ends by giving 'objective reality' to the most elementary classifications" (p. 200), seeing the Greeks as constantly occupied in the creation and admiration of perfectly beautiful statues or poems, the Romans as invariably austere, and so forth; where it is not shown how such classifications can have even instrumental value if they do not direct attention to certain objective facts, or what is the *formal* distinction between what is called a broad generalisation and what does happen invariably. However, it is not merely that we are left in vagueness on this particular point: the whole conception of "the individual" as the object of knowledge makes it impossible to show that one "universal" rather than another applies to it. A thing's "individuality" is the unspecifiable character which somehow comprehends and accounts for all its specifiable characters: but, in fact, *nothing* is accounted for along this line, and in place of knowledge we have only a welter of impressionism or subjectivism.

These considerations are reinforced by Croce's view of aesthetics. "The vital condition of art's autonomy", he says, "is simply the essential unity of the human spirit which, in its various activities, is never disintegrated so as to let each drift in isolation, but is itself always present as the pilot at the helm" (p. 133). This might be taken as at least an approach to the view that the same formal principles of criticism are operative in any field into which human beings might inquire — being in fact the principles of logic (or "categories"). But for Croce it is part of an attack on formalism and it appears to signify a unity of subject-matter, a treatment of all activities of the human spirit as *directed upon* the human spirit. It is in accordance with this position that Croce assimilates all arts to literature and that he has so notoriously upheld an expressionist aesthetic in which creator, appreciator and work are all run together. But, as with all subjectivists, he has to reinstate the distinctions on the "subjective" side and then arbitrarily to assert that they all belong to the unity of "self-consciousness". That this, in spite of his professions to the contrary, is not the way to overcome dualism can be seen when he says (p. 150), "Even when our thought studies and criticises the thoughts of others and traces their history, it is not strictly thinking about thought but about the practical activity of thinking, for thought is always the subject which thinks and not what is thought about." The untenability of a *theory* of an uncognised subject confronted with a cognised object is no less clear than its dualistic character.

The distinction and connection of types of cognition (e.g., "logical or ratiocinative" and "sensuous or intuitive") is no more successfully presented; nor does Croce give an at all coherent account of "the two grades of practical activity, the one egoistic or economic, the other universal or moral", which he says are both necessary (to practical activity, presumably) though the second continually subdues and refashions the first. "Since this solution of the problem we no longer think . . . of 'moralising economics', on the contrary we demand that it should assert its own nature yet more strictly, not as immoral but as non-moral, for that way is its function and its truth. At the same time we demand that the moral conscience should intervene to bring it under its supremacy, for human life is subject to this sole authority, which alone is competent to give the ultimate decision in moral conflicts by prescribing the action which may reconcile them" (pp. 29, 30). It is no solution of the antithesis, if there were one, between the good and the useful to make one the "natural ruler" of the other; this is only to deny the antithesis — but, besides that, Croce cannot establish a department of human life by the relational notion of "usefulness" (of being a means to something), he cannot, if good is qualitative, treat the good and the useful as complementary terms, and he cannot, if the question is of *two* relations (say, being commanded and being demanded), give any reason for taking either as subordinate to the other.

Croce's doctrines of the practical and the beautiful are, of course, familiar enough from earlier and longer writings; the present collection merely exposes them in their theoretical nakedness, not least by its insistence on that "amalgamation" or reconciliation according to which history has to be conceived in terms of "the abiding purposes of humanity", philosophers have to be democrats and anyone who recognises universality in ethics is a Christian (see especially the third essay "Why we cannot help calling ourselves Christians"). The Greek thinkers, we learn (p. 100), "never gave to the moral consciousness that emphasis and that pre-eminence which it gained from Christianity". But in fact it is pre-eminently in Greek thinkers that we find a disinterestedness and objectivity of outlook that can never be gained from a doctrine of personal salvation, which reaches its "highest" moral level in the notion of *altruism* — a notion still on the level of personal interest. Certainly Croce has here (particularly in the essay on "Justice and Liberty") some quite forceful criticism of modern egalitarianism, but this is counterbalanced by the absence of any real sense of objectivity, which would carry with it the recognition that "universal" does not mean "widespread" and that types of activity (or ways of life) of a distinctive ethical character can never be brought under the notion of "individuality".

To say that in the field of history "even the occasional defeats are the preludes of victories to come and of those advances in human thought and of [in?] civilisation which inexplicably accompany one another and continue from generation to generation" (p. 106) is to take a cheaply optimistic view which might well coalesce with religion but leaves without positive sense that "liberty" which is supposed to unify the historical process. It is a view having much in common with the doctrine of Marx, at whom Croce girds as a subordinator of all human values "to his one ruling interest, economic welfare and the social revolution" — an obviously biased judgment, since, whatever the defects of Marx's view of production, it is not a doctrine of "welfare" in the sense of personal comfort (it would compare more than favourably with Croce's many observations of a philanthropic sort, including the statement on p. 162 that the treasures of Christianity concealed in the religiosity of the Spanish or Neapolitan people "are sufficiently revealed by the cult of the compassionate Madonna") and it gives a more positive character to the conception of liberty than any doctrine of "inwardness" can do.

There is still much to be learned from Croce (as well as from Marx) on history and politics, though in this collection there is little on these subjects which he has not said more forcibly elsewhere. But there is nothing which can patch up the rickety philosophy of spiritual reality or the "circle of the spirit" (Croce's efforts only leave the gaps more glaring); the fundamental error is to take any concrete content as characteristic of reality as such. On the one hand, this leads to an evasion of the real problems of "spirit" (of mind and culture) in the assumption of a metaphysical "sanction" and support for forces which in fact have constantly to work and struggle with *alien* material — and, in setting up a bountiful reality, it is adopting a position no less "materialist" than those it attacks. On the other hand, in substituting for questions of *form* questions of "matter" or concrete content, or in amalgamating the two, it is cutting at the root of exact and detached (or "objective") thinking. Thus, while it may be conceded that Croce has opened up a number of philosophical questions, the general conclusion remains that in his anti-formalism he is anti-philosophical.

JOHN ANDERSON.

G. E. MOORE: A CRITICAL EXPOSITION. By Alan R. White. Oxford, Basil Blackwell, 1958. 226 p. 25s. (U.K.)

Mr. White's book, against its author's intentions, presents a curiously diminished Moore. The emphasis is on Moore's 'method', and Moore's reduction in size is due partly to this emphasis and partly to the absence of any historical setting to his work. (The book has a final chapter entitled 'The Historical Setting'; it deals with Moore's '18th century predecessors' — Berkeley and Reid — and his '20th century contemporaries'.) We see Moore not as liberating philosophy from the power of Absolute Idealism — two phrases exhaust White's references to Bradley — but as stumbling towards Wittgenstein and Ryle under the great load of his 'concept theory of meaning', his entitative universals.

Moore 'wrote little about his method . . . because his energies were given to practising it. For this reason and because I think that it is as a contribution to the right method of tackling philosophical problems that his work will have lasting importance, I give pride of place in my treatment to this method. Thus I attribute more importance to his 1900 article on "Necessity" than to the famous "The Refutation of Idealism" of 1903' (pp. 1-2). White quotes Moore's denial of 'a preference for any method', but maintains that this denial is compatible with his assertion that the search for analysis is Moore's characteristic 'way of doing philosophy' (p. 2). And he brings the article on Necessity into prominence because it contains so clear and so early a statement of Moore's pre-occupation with analysis. It shows that 'Moore's interest is not in finding either the truth or the meaning, in the sense of correct use, of what we say, but in finding the meaning, in another sense, of that about whose truth and meaning, in the first sense, there is no doubt' (p. 4). White does not suggest that this is Moore's only philosophical interest. His long discussion of the double meaning of Moore's question 'What does it mean to say so-and-so?' includes references to Moore's efforts to get at the meaning (in the 'ordinary sense') of *philosophers'* statements in order to get at their truth

Moore's search for analysis is less significantly called his method than the method of his search. How do we come to know the meaning (in the 'analytic sense') of something whose meaning (in the 'ordinary sense') we already know? White has assembled with meticulous care the materials Moore provided for an answer. For Moore it is a matter of being able 'to say what it is that we *see before our minds* when we see the meaning of an expression, that is, the notion conveyed by it — with perhaps the hint that what we see is the property common to all those things in regard to which the expression is correctly used; or being able to say what are the elements, the constituent concepts, into which the notion or concept can be *divided* and which, as it were, compose it; or being able to say how the given notion is related to and *distinguished* from other notions which are conveyed either by the same or by different expressions' (p 66). White stresses in each case the 'being able to say'

The view that analysis is a matter of inspection is Moore's 'basic view', and the one most closely connected with his hypostatization of concepts, and the one, in White's opinion, most radically mistaken — you don't advance towards an analysis by peering hard at something hard to see (pp. 66-72). When Moore talks about analysis, at least when he talks about it under that name, he talks about division; "his practice largely consists in the work of analytic distinction" (with inspection playing a subordinate role); so that, as far as analysis is concerned, he is related by his words 'to Russell, the Vienna Circle, and the early Wittgenstein' and by his work to the later Wittgenstein and to Ryle (pp. 74-75). White is all for the 'distinction' view of analysis: besides its other virtues, it is prophylactic against Moore's obsession with 'translation' and synonymity.

The discussion of Moore's opinions on analysis is surrounded by chapters dealing with his opinions on common sense, ethics and perception. White successfully disposes of the too clever interpretations of Moore which identify his defence of common sense with a defence of ordinary language. The chapter on Moore's handling of the problems of perception is a beautifully lucid and concentrated piece of exposition and criticism. White ends it as an advocate of sense-data, but not as Moore's advocate. ('I have tried to show that there are sense-data, but that they are not what Moore thought they were', p. 190.) The question 'What do you see?' has two types of answer. 'A penny' answers it and so does 'Something brown and elliptical', but quite differently. The first answer identifies what the second describes. Sense-data language is descriptive, material-object language identificatory. On the basis of the distinction between identification and description White builds up a theory of sense-data which is worth the attention it cannot have in this brief review of his whole book. There is one thing which anybody who gives it his attention will have to consider, and that is how far White means by sense-data what other philosophers have meant. 'For "visual sense-datum" is only a generic word for whatever gives a description answer to the question "What do you see?"' (p. 189).

S. A. GRAVE.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

Bowes, Pratima. The concept of morality. London, Allen and Unwin, 1959. 220 p. 21s. (U.K.)
A defence of objectivism against such moral theories as those of Stevenson, Hare, Nowell-Smith and others.

Cleobury, F. H. Christian rationalism and philosophical analysis. London, James Clarke, 1959. 142 p. 15s. (U.K.)
A defence of theology in terms of linguistic analysis.

Goussinsky, B. Continuity and number. Tel Aviv, the Author, 1959. 31 p. 50c. (Paper covers.)

Grene, Marjorie. Introduction to existentialism. Chicago University Press, 1959. vii, 150 p. 10s. 6d. (U.K.) Paper covers.
First published in 1948 as *Dreadful Freedom*

Johnstone, Henry W., Jr. Philosophy and argument. Pennsylvania State University Press, 1959. 141 p. $4.
It is hard to see how a fundamental philosophical thesis can ever be disproved, since nothing could count as evidence against it. Nevertheless, philosophical arguments have more than merely persuasive force, and can be valid or invalid, though a valid philosophical argument is always, in a sense, an *argumentum ad hominem*

Lewis, H. D. Our experience of God. (Muirhead Library of Philosophy.) London, Allen and Unwin, 1959. 301 p. 30s. (U.K.)
Discusses the meaning and justification of religious assertions, from the point of view of one concerned 'at the spread of religious indifference and the secularism which springs largely from despair . . . of being religious without sacrifice of intellectual integrity".

Melden, A. I. Rights and right conduct. Oxford, Blackwell, 1959. 87 p. 20s. 9d. (Australian). Paper covers.
"My ultimate objective . . . is to invite attention to relatively neglected issues in moral philosophy . . . My immediate objective is to explore some of the so-called foundations of certain familiar moral rights" such as the right of a parent to special consideration from his child.

Moore, G. E. Philosophical papers. (Muirhead Library of Philosophy.) London, Allen and Unwin, 1959. 325 p. 30s. (U.K.)
Eleven papers, written between 1923 and 1955. Two ("Certainty" and "Four Forms of Scepticism") have not been previously published.

Runes, Dagobert D. A dictionary of thought from my writings and my evenings. New York, Philosophical Library, 1959. 152 p. $5.

Russell, Bertrand. My philosophical development London, Allen and Unwin, 1959. 279 p. 18s. (U.K.)
A new book, *not* a collection of already published writings (except for a final chapter containing four polemical articles on contemporary Oxford philosophy), this is a complete philosophical autobiography,

starting from Russell's early Hegelian period and including two chapters on *Principia Mathematica* and a discussion of "The Impact of Wittgenstein". It ends with an Essay by Alan Wood on the development of Russell's philosophy, which would have been the Introduction to a new technical study of Russell's work, had the author lived to complete it

SANGHARAKSHITA, Bhikshu. A survey of Buddhism. 2nd edition Bangalore, Indian Institute of World Culture, 1959. viii, 527 p 27s. (U.K.)

ULLMANN, Richard K Between God and history. the human situation exemplified in Quaker thought and practice London, Allen and Unwin, 1959. 212 p. 21s (U.K.)

WEBSTER, T. B. L. Greek art and literature, 700-530 BC: the beginnings of modern civilization. (de Carle lectures, University of Otago, 1959.) Dunedin, Otago University Press and Melbourne University Press, 1959. xviii, 125 p 25s. (Australian) Paper covers

WILSON, N. L The concept of language. University of Toronto Press, 1959. viii, 153 p. Price not given

A serious attempt to treat a language as a system of rules raises the question: What is a language? "There exist no general working definitions for semantics comparable to Peano's working definitions for arithmetic" This book sets out to provide them, and to define such terms as "true", "logically true", "designates". The author concludes that "the world has a certain structure and in one way or another it imposes this structure on languages". A fairly advanced knowledge of symbolic logic is presupposed

YARNOLD, G. D The spiritual crisis of the scientific age London, Allen and Unwin, 1959. 207 p. 18s. (U.K)

"Scientific explanation, 'from below'. must be supplemented by something far wider and deeper, interpretation 'from above' . . . Western civilization above all things needs to recover its hold upon the Christian verities" Foreword

NOTES AND NEWS

AUSTRALASIAN ASSOCIATION OF PHILOSOPHY: ANNUAL CONGRESS, 1959.

The Congress and Annual General Meeting for 1959 were held in Adelaide from August 21st to August 26th. The following papers were read:

Friday, 21st August: Presidential Address Professor A C Fox — "Another Look at the Concrete Universal".

Saturday, 22nd August: Professor J Passmore — "The Infinite Regress Argument".
Mr. R. S. Walters — "The Problem of Counter-factuals".

Sunday, 23rd August: Dr. A C Jackson — "Intentional Action".
Mr. B. Ellis — "Measurement"

Monday, 24th August: Mr. J. E McGechie — "Implication and Intention".
Mr. D H. Monro — "Universalisability".

Tuesday, 25th August. Professor J. Mackie — "Equality of Times"

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I should like to acknowledge with thanks receipt of gifts of rare issues to the Association recently from: Mr. Nicholls, Mr Rose and Mr. Roxon of the University of Sydney; Mr. Bradley and Mr Walters of the University of New South Wales; Professor Kyle and Dr Dowling of the University of Queensland, and Mr. J C. Begg of Dunedin, New Zealand.

D C Stove,
Hon General Secretary

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An International Congress for Logic, Methodology and Philosophy of Science will be held at Stanford University, Stanford, California, U.S.A., from August 24 to September 2, 1960, under the auspices of the *International Union for History and Philosophy of Science.*

Information about membership fees and other details of the Congress may be obtained by writing to Professor Patrick Suppes, Serra House, Stanford University, Stanford, California, U.S A.

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